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REVERSE ILLUSIONS OF ORIENTATION.*

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I use this phrase to designate a group of facts which have not yet been studied methodically, but which have been cited by many authors and described in different words. These facts consist of illusions or hallucinations of orientation, which arise spontaneously either when we waken in the darkness of night, or during the day when awake. These illusions do not appear to be experienced by a great number of persons, as is shown by the fact that nearly all the individuals whom I have questioned for a long time have not been able to discover in their past experience any instance of this 'reversal' of orientation.

The first indication which I find is in a letter of Henry Forde, published in *Nature* (Aug. 17, 1873) following a communication of Darwin published in the same journal (Apr. 3, 1873). The following is Forde's letter: "In the wild parts of the state of West Virginia even the most experienced hunters who frequent the woody mountains of this wild region are subject to a kind of shock, so that they suddenly lose their heads and feel that they are going in a direction directly contrary to that which they intended to follow. It is useless for their companions to reason with them and show them the position of the sun. Nothing can conquer this feeling, which is accompanied by a great nervousness and by a general sensation of distress and of uncertainty. The nervousness comes only after the seizure, and is not the cause of it."

* *Le Renversement de l'Orientation.*

These observations are also cited in an article of M. Viguier,* who has added no personal contributions. I myself have published some observations upon this point in *Mind* (1884), in an article entitled *Vertigo of Direction*. This title is very inexact since there is never produced, at least to my knowledge, an illusion of a movement of objects, or a true feeling of vertigo. Again, M. Flournoy, in his recent work *Les Synopsies* (p. 188), speaks incidentally of these phenomena, which he has observed in himself at the moment of waking. "Who has not happened," says he, "to waken in the darkness of the night with the curious idea that the room is turned in some way. We recall ourselves, and know certainly that we are lying in bed with the right side toward the wall, and in spite of this we feel that we have the wall to the left and the room to the right and are amazed at the tenacity of this illusion, which remains for many seconds in spite of reason; until, extending the arms, the contact with the wall causes it suddenly to vanish and brings the mental images back to their proper position. It is to be presumed that this reversal at awakening from sleep is due to the prolongation of a dream in which one has thought himself in another chamber."

I may now report the observations which I have collected.† I shall follow these by my personal observations, and then review the whole by presenting a general description of the phenomena in question.

OBSERVATION I.

(By H. BEAUNIS, Professor in the Faculty of Medicine at Nancy; Director of the Laboratory of Psychology at Paris.)

"Formerly I often made the trip from Paris to Nancy, and each time I observed in myself the following illusion, very difficult to describe in a way to make it easily comprehensible to the reader. On leaving Paris I had, as every one has, a sensation of going in a definite direction; in other words, the direction of my passive movement sustained a definite relation to the landscape which the train traversed.

* *The Sense of Orientation and its Organ in Animals and Men* (Rev. Phil., July 1882).

† These observations have been collected among my immediate associates: they come from persons whom I see daily, whom I have long known, and in whom I place all confidence. All these persons have habits of close observation.

This sensation remained the same, fixed, until at a short distance from Nancy. Then suddenly, at a given instant, at the moment when I turned my eyes to the landscape, I had the sensation of a sudden change of direction, or rather I had a clear sensation that the relation of the landscape to the direction of my passive movement had suddenly changed. I cannot better express the sensation which I felt than by the expression 'being turned around' (*renversement*). It seemed to me that the landscape turned itself about, and that I travelled in a direction opposite to that which I followed on leaving Paris. This sensation was produced equally whether I rode forward or backward. I was living at that time in Nancy very near to the railroad, a little in front of the station; and the sensation of being turned around was produced only when I reached a region which was entirely familiar to me.

"This sensation was not produced when I made the journey in the opposite direction, i.e., from Nancy to Paris. It was not caused when I travelled from Nancy to Strassburg or from Strassburg to Nancy—a journey which I seldom made, however.

"The principal point of explanation, in my opinion, is that the sense of being turned around was produced only when I entered a region in which the smallest details, the roads, the houses, the gardens, etc., were familiar to me; and at the precise moment when I laid my eyes on this landscape. I have at different times tried to hold my eyes fixed on the landscape for a long enough time to see whether and by what means this sensation would be produced. But fatigue very soon obliged me to withdraw my gaze, and fix it upon the immovable parts of the interior of the car; and the sensation appeared only at the moment when my eyes moved from the car to the landscape.

"A further explanation is this, that, owing to the location of the two stations at Paris and Nancy, the train in starting from Paris goes from south to north, while at the arrival at Nancy it is directed from north to south (approximately). It is possible that this change of direction impresses me only when I arrive at a region all the details of which are known. I should add that my faculty of orientation is not well developed."

We remark that M. Beaunis, in trying to describe the impression felt, has used precisely the word (*retournement*) employed by Forbes, although he had no knowledge of the description of the latter. M. Beaunis cannot explain the illusion. He states that it is always produced in the same external conditions, during a railroad journey and at the arrival of the train at a certain place.

OBSERVATION II.

(By M. JACQUES PASSY, Chemist.)

This second observer gives to the phenomena the name of disorientation, i.e., not a total loss of sense of direction. We shall see that it deals with precisely the same illusion, although the conditions in which it is produced are different.

"I clearly recall three cases of disorientation, together with the cause which produced them. But the effect has been produced oftener, perhaps nine or ten times altogether, without my having remembered the details. In general, I possess a good enough sense of direction; for it guides me in dense woods or in Paris. I also remember very well the names and appearance of streets.

"I was proceeding toward the Latin quarter, and had to cross the Rue de Reine, with the Mont Parnassus station on the right, the Boulevard St. Germain station on the left; and I proceeded with the idea of coming to the Rue de Reine. Probably lost in thought, I crossed this street without perceiving it. I continued my way persuaded that I had it before me. At the end of some moments, not recognizing where I was, I retraced my steps, reading the names of the streets. I came to the Rue de Reine with the station on the left, the boulevard on the right. My confusion was extreme, and I found myself for some time in a state of complete disorientation, and unable to comprehend the situation. This state did not at all resemble that which is produced in an unknown place—in a forest, for example, where one has gone astray and does not know where he is. Here you know very well where you are. You have a very clear sense of direction and you know perfectly where things ought to be; only this direction and this place are just the opposite of their real position. The station of Mont Parnassus ought to be on the right, and I did not understand why I did not see it there. What we have then in this particular case is a completely false orientation. All the objects and streets occupy positions exactly opposite to those which they should. When you recognize the objects you realize that you are under an illusion, but the illusion does not disappear. I remember that while a child, returning home one day from the Bois de Boulogne, *I* asserted to those who went with me that we were following a road exactly opposite to the right direction, and I found the direction which they forced me to take absurd to such a degree that I wanted to cry, and my error disappeared only when near the house. At the end of a certain period of conflict, three or four minutes perhaps, the illusion

disappeared. And when it disappeared for one street or one object, it disappeared for all. I could then immediately take the road and direct myself correctly. Further, the same illusion is produced in identical conditions. I crossed, in the same way, the Boulevard Sébastopol without perceiving it, and the illusion was of exactly the same nature.

"Very lately, coming from the Place de la République, I went to a house situated about the middle of the left side of the Rue de Temple. The person whom I went to see lived in a court with many entries. My visit ended, I started to go out, when, all at once, instead of taking the right to return to the Place de la République, I took the left toward the Hôtel de Ville. How did I realize my mistake? Whether it was a simple distraction, or whether I had lost in the court the sense of direction, or had simply not recalled whether the house was on the right or on the left, I do not know. It is certain that while on my way I felt sure of meeting the Place de la République. Thus my confusion was extreme on coming to the Hôtel de Ville. As before, I was some moments in recognizing it. Then I recognized the Hôtel de Ville, without destroying the illusion. It disappeared, however, very quickly, and, I think, like the others, when I understood the cause of my mistake.

"In all the cases that I have cited, the imaginary direction was exactly opposite to the real direction. I do not recall having an impression of a direction at right angles to the real direction. We often learn of errors arising from failure to estimate angles; but these easily rectify themselves without giving place to anything which resembles vertigo, of which I have just cited three cases."

OBSERVATION III.

(By M. VICTOR HENRI, Student of Science.)

I now record two observations of exactly the same kind as those which precede. The details alone are different. I attach particular importance to them because they come from a man who has the habit of analysis.

"First Observation.—In the month of September, 1893, I went with some people to the bridge of Austerlitz to take the boat and go either to Charenton or to Auteuil. This had not been decided on beforehand. On coming to the bridge we decided to go to Auteuil, and we took the boat on the left side of the Seine. The boat was turned in the direction of Charenton. We sat down in the cabin at the back of the boat. I did not notice how the boat was turned. In passing

before the church of Notre Dame I saw it, for the first time, through the cabin-windows. I had a strange feeling akin to astonishment. I looked again and again, as if I did not see well, or as if I did not believe what I did see; for it seemed to me we were going in the direction of Charenton and not to Auteuil. When I was not looking through the window it seemed as if we were going to Charenton. Looking out, I was obliged to admit that I was deceived. It is a very painful feeling. When I went outside I saw with astonishment that the Palais de Justice was on the left and not on the right. When I descended into the cabin the illusion disappeared.

"Second Observation. (Made on the morning of January 1, 1894.)—In my chamber the bed is in front of the window. I have to lie on my *left* side to see the window. The curtains are thick and let in very little light. In the evening I lay on my left side, and do not recall having turned during the night. I slept well. In the morning I awoke, opened my eyes, tried to see if it was day, and looked fixedly before me so as to see the window. But I could see nothing at all, not a trace of light. Then, *suddenly*, I had the vague feeling that something was wrong, only I knew not what. I said to myself, 'I am lying on my right side: the door is there [I indicated the opposite direction], so that I ought to see the window before me; but I do not see it.' Then I made a movement with my hand and touched the wall. I felt it with astonishment, recognizing that I was very much deceived. Turning, I saw the window on the other side, and felt at the same moment that I had deceived myself as to the direction of the door. During the night I had turned over without knowing it. The result was the same, therefore, as if the bed had been turned 180°."

OBSERVATION IV.

(M. PHILIPPE, Chief Assistant in the Psychological Laboratory at Paris.)

This observation differs from the preceding in that the illusion of direction which is here described is not relative to the objects present, but to objects represented in memory.

"When I think of a place, a street, a monument in Lyons, I myself being at Paris, I picture to myself a map of Lyons turned around. A certain side of Lyons is nearest to Paris, that by which I enter Lyons in going *as the crow flies* from Paris to Lyons. I invariably picture Lyons in such a way that the wrong part of it is brought around in the direction of Paris. . . . This habit of orientation takes such hold upon me that I prefer to arrive in Lyons at the depot on the side which I

picture toward Paris, although the other depot would be more convenient."

OBSERVATION V.

(M. COURTIER, Demonstrator in the Psychological Laboratory at Paris.)

In the following observation the illusion is very slight and it is not certain that it is of the same kind as the others.

"One night I took the train from Rouen to Paris. At the end of a half-hour I fell asleep. At Vernon I suddenly awoke and, going out, walked a few moments on the platform of the station. After entering the car again I had the sensation of returning to Rouen. It seemed as if I were going away from Paris, although I took the same seat. There were at the station, the instant when I descended, two trains from opposite directions; and although I had not crossed the track, and although I did not think I had crossed it, I asked myself for one or two minutes whether I had not been deceived in the train. I was somewhat anxious, and found it necessary to ask my neighbors as to the fact. When I had been assured that we were going toward Paris, the illusion still persisted for some minutes. It was half-past nine in the evening and very dark. I have made this trip a hundred times in ten years, and have suffered the illusion but this once."

OBSERVATION VI.

(By M. P. THÉLOHAN, Chief of the Histological Laboratory at the Collége de France.)

"I have often felt complete change of direction on the railroad or in a carriage. It has been generally on waking from a more or less profound sleep, or after some degree of drowsiness, that I have had this illusion. For example, being asleep in the car, I was at a given moment awakened by the arrival of the train at a station, and by the noise in the depot. Then sleep began to seize me. The departure of the train caused an awakening more or less complete, and it then seemed to me that the direction of the journey was absolutely changed. I remember very clearly having had under these conditions a moment of very disagreeable anxiety, of real pain. It was only after some moments that, after thoroughly waking, I recognized my error, by comparing the relative situations of the different objects."

This observation seems to be comparable with those of MM. Beaunis and Courtier.

OBSERVATION VII.

(By M. ROBB, Bachelor of Science.)

Although very succinct, this observation puts before us very tersely the fact that during the derangement of orientation we may cease for a moment to recognize familiar objects.

"One day I was going toward the Rue Royale from the side of the Rue de Provence. At the Madeleine, instead of going to the left as I should have done, I turned to the right. Reaching the Rue Tronchet, I did not recognize either the street or the gate of the Madeleine. Two or three minutes passed before I discovered that I had taken the wrong direction."

OBSERVATION VIII.

(By M. PORTIER, Demonstrator in Physiology at the Sorbonne.)

This curious observation suggests that of M. Philippe.

"I arrived for the first time at Croisic one morning at break of day. I had slept in the train on leaving St. Nazaire, and it was only a few moments before arriving at Croisic that I was wakened by the voice of one of my travelling-companions. He showed us the sea just coming into view in the distance. It was at this moment that from the direction of the river (only a small part of which I saw) and from the general direction of the railroad I deduced the direction of the north and the sea. I left the train with this orientation. Croisic is situated on a peninsula, so that in going through the town and its surrounding country I often saw the sea in different directions. I then chose unconsciously that one of the directions which coincided with the orientation which had been established in my mind at the moment I left the train. I repeated this many times, visiting Croisic and its surroundings, and comparing everything with this orientation. I discovered that this was false by seeing a boat start out from the pier for a certain destination which I thought lay in quite the opposite direction.

"By means of a compass I corrected the error. During the rest of my stay at Croisic I had two very distinct orientations: the first, the false one, which had imposed itself on my mind, persisted in spite of me; and the second, the true one, which I found only at certain moments thereafter by reflection with a marked effort of will. When I was in the streets of the town or in a house, I suffered no annoyance from this fact; but when I made an excursion into the surrounding country, or when some one mentioned to me the name of a distant

place, Belle-Isle, England, New York, I suffered very great inconvenience in representing the place to myself in its true situation. This annoyance, this indefinable distress, slightly comparable to that which one suffers when dizzy, persisted during the whole time of my stay at Croisic (about a month and a half). This state left an impression in my mind so disagreeable that now, whenever I arrive in a place which I do not know, I take care to orientate myself properly."

M. Portier, in trying to show by a sketch the false orientation of which he speaks, compared to the true, finds that they are not exactly the reverse of each other, but show a relative displacement of from 60° to 100°.

OBSERVATION IX.

(By the Author.)

I have had these illusions of direction for a number of years. The first which I remember dates from 1876; and as lately as three weeks ago (March 1894) I experienced it again. I have taken note of about a dozen different cases which have occurred to me—all very similar. From 1880 to 1882 I often visited the Louvre and became quite familiar with the halls of painting and sculpture. But I have never taken pains to keep my direction in these rooms, and have made very many *détours* to right and left; so that, regularly, at the end of a quarter of an hour I lose the sense of direction entirely. The majority of the rooms receiving their light from above, or being lighted by windows opening into an interior court, one has very few indications from which to get one's direction. Further, I was careful not to keep any direction, my attention being concentrated upon the works of art. Now in these conditions this is what I observed on one occasion. By chance my walk brought me into a hall of which the windows opened upon the Quai de Seine, known to be on the right. I approached the window in order to look at the Quai a moment, and then suddenly I had a feeling of 'reversal.' I saw the Seine rolling before me from left to right; but it seemed quite wrong, for in the position in which I found myself the Seine ought, as I thought, to roll in the opposite direction: the landscape seemed to be turned around. This was a common experience. Often, after several intervening days, I have felt this particular illusion reproduce

itself, accompanied by a painful sensation. Then I ceased to frequent the Louvre, and I did not have another opportunity to observe this. A year ago I tried it again. Going to the museum I made no effort to keep my direction, and at the end of an hour of walking approached a window opening upon the Quai de Seine, but the illusion was not produced.

About eight months ago (August 1893) I went to walk with my family in the forest of Fontainebleau. We abandoned the roads and turned into the woods to observe the lake (Mare des Marches). The sun was hidden by foliage, the atmosphere was obscure, and we had no known point of reference from which to get our direction. But we had left the road at a crossing where two roads met, one of which goes off to the Bois-le-Roi, and the other to the Corbuisson. These two roads intersect at a little less than a right angle, and, as we knew that the Mare des Marches occupied a region situated approximately equidistant from the two roads, we followed, in a straight line, the bisectrix of the angle formed by these two routes. At the end of a rather long walk which lasted nearly a half-hour, we came to a road without a guide-post—no indication of any sort. We found that this road ran into another on our right. We then discussed our position; one lady and myself expressing the opinion that the route continued to the right and ended at the Corbuisson, and we thought we could recognize the latter in the distance. Our conviction on this subject came from the idea that while we were walking through the woods we had followed a straight line, having the Corbuisson at our right and the Bois-le-Roi on the left. One lady of the company thought, on the contrary, that we had not walked in a straight line through the woods, but that we had described a semicircle toward the left, and the road followed to the right, would lead to the Bois-le-Roi. Two other persons, gentleman and lady, had no opinion. Testing the matter we found the road on the right to be the Bois-le-Roi. My lady companion and myself were therefore entirely wrong; but after verifying our position the illusion persisted. We could not drive out the idea that the Bois-le-Roi was occupying the position where the Corbuisson ought to be. Even to-day when I take this walk, I imagine the direction to be as I first conceived it, although

I know now that it is wrong; and it is only as I go on that, little by little, the sight of familiar objects corrects my error.

Several years ago I went one evening to attend a conference in the mayor's house at Neuilly. I then lived at Neuilly, and my road in coming back was to take the avenue which runs from the Pont de Neuilly to the Arc de Triomphe. In returning along this avenue, after leaving the mayor's house, I had a clear sense of being turned around. On coming into the avenue I had the Arc de Triomphe on my left hand and the Pont de Neuilly on my right. It seemed to me that as if by a stroke of a magic wand the Arc de Triomphe and the bridge had changed places. The illusion was accompanied by a very disagreeable feeling of distress—a state carrying with it a slight degree of stupor, as is expressed by the English word *amazement*. If I had been alone I do not know that I should have been able to find my way. The illusion disappeared by degrees, and, once over, I tried in vain to reproduce it in memory. This case impressed me very much, and I immediately wrote a description of it. It is exactly as if the world turned around with a man as a centre: when he opens his eyes he finds all the articles in his room displaced 180° by rotation.

The two following examples of this illusion contain each a very interesting particular. In an omnibus in Paris about four months ago I thought I recognized my terminus in the distance, a station situated in a street which ran toward the Seine. I was deceived. The street in question looked toward the west. I thought, therefore, that the street which looked toward the east did really look toward the Seine: an error of 90°, and not of 180° as in the earlier cases.

Tuesday, March 21, I took the omnibus of the Odéon to St. Sulpice. It was eight o'clock in the evening. I sat in the back on the right, so that the horses were on my right hand. When the omnibus arrived in the Rue de Richelieu I was conscious of a gradual turning of objects about me. Then I had the feeling perfectly distinct that the omnibus was going in the opposite direction (i.e., away from the Boulevard des Italiens, which is at right angles to the Rue de Richelieu). I was only a moment under this illusion. I was not the dupe of

it, because I knew very well that the omnibus could not have taken the opposite direction without my perceiving it. I was very calm, and having been studying this phenomenon for some months I was very glad to get it again. I can now make this illusion artificially by thinking a moment of the state of the omnibus. Further I find myself incapable when I go back to it in thought, of imagining the omnibus going in the right direction. I find myself again in exactly the same mental situation, and I again experience the same illusion without being able to correct it. To continue the account: I descended with my friends to the station of the Boulevard des Italiens, and we were directed toward the Théâtre du Vaudeville on this street. To this day, although I am perfectly acquainted with this quarter, I am no longer able to keep the direction of the Vaudeville. When I think of a certain point in our journey, the illusion returns. I recall a magazine of arms illuminated by electric light; and I again see it with the false localization which I attributed to it then. The illusion disappeared little by little, without my losing consciousness of what was going on in me. I have not been able to understand how the rectification was made, yet I feel myself capable not only of reproducing the illusion, but of reproducing the rectification. On arriving before the Vaudeville, things were readjusted after a moment. The illusion had lasted in all nearly three minutes. It did not appear again for the rest of the evening nor the next day.

I have reported this observation at considerable length because it is the second instance in which the illusion of orientation was produced while the body remained unmoved. I was seated immovable in the omnibus for about a quarter of an hour: that is, I did not change my seat. This case then recalls that of M. Beaunis.

It remains to attempt some explanation. As all the observations have clearly indicated, the illusion rests upon the orientation of objects. One observes upon his right, for example, the objects which, the actual position of the body of the observer being given, ought to be on the left; or one perceives before him objects which should be behind. This is the basis of the illusion in all the observations which we have recounted.

XUM

We can distinguish three cases: (1) normal orientation—in which the points of reference recognized confirm the former sense of direction; (2) disorientation—one has no sense of direction at all, and if he meets a familiar point of reference he accepts it and orientates himself properly; (3) inexact orientation—one meets a point of reference, finds it in contradiction with his earlier system; the false system persists, even though he knows it to be false, just as an illusion persists. This last is the case now under discussion.

As to the force of the illusion, the experiences are slightly varied. Sometimes the illusion is relatively slight, and one recognizes familiar objects and is conscious of the contradiction between the two directions suggested. In one of my own cases, however, the illusion is so vivid that one has difficulty in recognizing objects well known and familiar. The illusion is accompanied almost always by distress and anxiety.

Generally the illusion is equivalent to the effect of a rotation of 180°, and that is why most writers compare their impressions to a turning or 'reversal.' But we have cited two cases in which the reversion of objects appeared to be only 90°. The illusion appears in the most diverse circumstances: in the waking state, after a walk, or during rest on the railroad, boat, or omnibus. The active movements of the body are not, then, necessary for the illusion. The most important thing to consider is that the illusion can be led up to by conscious or unconscious judgments. In one of the observations of M. Passy we have a very striking example of it: in coming from the Bon Marché he had to cross the Rue de Rennes; he was consequently convinced that by crossing this road he would have the Mont Parnassus station on his right. It was this false orientation by reasoning which imposed itself upon him in spite of the contradiction of facts, and created the illusion. And the same explanation appears to be suitable for one of the cases which I report—that of the forest of Fontainebleau—and equally to the case of M. Portier. Perhaps the remark which M. Beaunis makes in regard to the stations at Nancy and Paris may explain his illusion. In other circumstances we may suppose, with some appearance of truth, that the subject has made a subconscious or even quite uncon-

scious orientation of objects, and that it is this which, when brought into conflict with actual perceptions, produces the illusion. Thus M. Henri, not perceiving that the boat had turned, unconsciously orientated the objects about him as if the boat which carried him had been moved only in a straight line. What was curious in this case was that the observer took no pains to orientate himself. The orientation was made automatically, while he was occupied with something entirely different. We can, by hypothesis, extend the same explanation to my personal case in the Louvre, supposing that during my promenades in the halls I had made an unconscious orientation of the objects. I localized the north in such a direction without thinking of it, and when I looked out of the window I was seized with the sense of its incorrectness. But there are cases in which the illusion appears suddenly, without any one being able to give account of the mode of production (for example, my personal case last cited, that of M. Thélohan, etc.). The anxiety which accompanies the illusion is explained by its gravity; it extends to the relation of all objects to the body. It has been remarked, and I think very justly, that the state of anxiety follows the illusion and does not produce it.

We still need to know whether the illusion is produced or not by a particular derangement of one sense-organ—possibly the semicircular canals of the inner ear. In experimental studies that have been made on the sensation of vertigo, no one has, to my knowledge, produced such illusions of the orientation of objects. The question is interesting and certainly deserves more study.*

* Translated from the author's MS. by J. N. Dodd, Fellow of Princeton College.

DIRECT CONTROL OF THE RETINAL FIELD.

BY PROFESSOR GEORGE TRUMBULL LADD,

Yale University.

There are not a few problems in experimental psychology —and some of these by no means the least interesting and important—which require no apparatus and comparatively little expenditure of time. Among such, one was brought prominently to my notice several years ago while endeavoring to discover how far visual dreams depend upon the arrangement of the light- and color-spots in the retinal field. (See *Mind*, Second Series, No. 1.) As, at first, an incidental affair, I then found out that their arrangement could, in my case at least, be brought under control of the will. This power grew rather rapidly with continued practice. That is to say, I was soon able, by attentively willing for perhaps some three to five minutes, to cause a cross, or a circle, or two concentric circles, or some other simple figure, to appear in the retinal field.

I have recently been experimenting with a class of sixteen advanced students—three seniors and the remainder graduate students—to see whether this power of control over the retinal field is at all common. The results have been exceedingly interesting and, in some respects at least, decisive. Let it be understood that all these experimenters understood the problem perfectly well and, having studied psychology from two to six years, were quite competent to answer it intelligently. What they were asked to do was briefly this: to close the eyes, allow the after-images completely to die away, and then persistently and attentively *to will* that the color-mass caused by the *Eigenlicht* should take some particular form,—a cross being most experimented with. They were to notice the effects of time and fatigue, and were also to see whether the

color as well as the form of the object thus willed was at all under control. It should be borne in mind that here was no question of the effects merely of imagination, or of visual hallucinations projected into space. The primary question was, *Can the retinal sensations which arise with the eyes closed and motionless be made to respond to volition with respect to the form and color which they assume?*

The results obtained were as follows: Of the sixteen persons experimenting with themselves, four only reported no success; nine had a partial success which seemed to increase with practice and which they considered undoubtedly dependent directly upon volition; and with the remaining three the success was marked and really phenomenal. It should be said, however, that of the four who reported 'no success,' only one appears to have tried the experiment at all persistently.

The nature of the partial success attained by nine of the class can best be understood by quoting from my notes of the reports made to me. Miss C. at first had no success at all. But by persistent trying, lines corresponding to the limbs of a cross began to appear in the retinal field; and once she got a complete cross. This experimenter thinks that her failure at first was largely due to looking too near; for several times, after she had tried in vain for some minutes, the limbs of the cross would suddenly start up in the distance, as it were. Miss C—n succeeded two or three times in getting the vertical bar of a cross which would remain for a second or two, then scatter, and then gather again. Mr. M., too, not infrequently succeeded in getting the vertical bar, but could only get the horizontal bar in a 'flickering' way; and then, while he was trying to make it lie over the other stationary bar, it would disappear. In his case the effect of fatigue in diminishing success was quickly apparent. Mr. S. also succeeds invariably in getting the vertical bar of the cross, but finds the horizontal bar more coy and flickering. The color of the figure in his case is uniformly light at first, and then changing to dark.

Mr. M—y reported that at first he could do nothing in the way of making the desired cross appear. But by persistent

effort he soon succeeded in making two complete dark crosses arise in the retinal field, one of which he thought he traced to the retina of each eye. A yellow upright cross with a short horizontal bar, on one occasion, started up at apparently about two feet distant. Mr. C. also, on practice, could produce at will a perfect cross, with the vertical bar darker, which would after a brief time disappear and then reappear with the bars of a complementary color. Mr. D., like several of his colleagues, had on the first trial no success whatever. But, on the second trial, he by persistent willing developed a vertical bar which remained stationary; while the required horizontal bar, although it appeared to order, persisted in sliding up and down the vertical bar. The third time, however, he got a perfect square cross which finally changed to an × shape. He could also obtain a circle at will. This experimenter found that, to obtain the best results, he must not be fatigued by more than five to seven minutes of trial; and that frequently the desired image 'jumped' into being all of a sudden when he was about to cease trying. Of the three others who had a partial success, one could sometimes get the vertical bar; another could get both bars separately, but could not get them to cross; and the third could generally get an upright and perfect cross within three minutes of beginning to try.

I have reserved the three most remarkable cases for a somewhat more detailed statement. Of these three Miss S. is very extraordinary in her control over the *color* of the object produced at will; her control over the shapes is less complete. An unusual color-sense appears, indeed, to be congenital in the family, having been possessed by the father and several of the children. This includes the power to match very delicate shades of color, almost infallibly, by memory. At first Miss S. obtained only a 'flickering' cross; but after trying daily for several days, she became able to get this figure, or a circle, every time at will. The period ordinarily required for the complete control of the effect was from ten to twenty minutes of as steadfast willing and waiting as was possible. Then at least one particular form of a cross could be got, at will, in all the principal colors *except red*. The violet and the purples were, however, easiest to obtain. An intensely bright but not

a dark blue could always be got; the green, on the contrary, was always dull, and the yellow and orange imperfect and tinged with brown. The violet cross was especially brilliant and seemed to start into the field suddenly, after the requisite period of trying. At my suggestion, the eyes on being opened after these voluntary crosses were obtained were immediately focussed on a sheet of white paper, and the cross found to appear on the paper in the complementary color.

Mr. B—r is also somewhat unusual, not to say abnormal, in his powers of vision. His eyes have some defects, especially in localizing promptly and correctly. His success is best at night, when a perfect cross or circle can readily be produced at will. The color-mass which the volition arranges into this shape is usually of a grayish color. The form generally appears to be located very near; but it may at will be set at a great distance, and then the cross appears as though seen through a long tube. Upon the cross a circle can be projected at will, which is usually best secured when the intensity of the color-mass is 'moderate.' This circle can then, by somewhat persistent willing, be separated from the cross and located near the bottom of the vertical bar on either side, but with varying degrees of difficulty.

A square cross is generally selected as 'most pleasing'; but the cross can be made to assume the figure of an ×. This experimenter noticed that the strong light from a gas-jet falling on closed eyes is not an uncontrollable color-mass; but a definite brilliant cross can be planted within it by an act of will. Mr. B—r finds that the effect of fatigue is to make the cross persistent in the retinal field; and this effect is sometimes so marked that he is obliged to open his eyes in order to dispel it.

Mr. D—s, who has studied art considerably, has a very vivid color-sense. He can produce a perfect cross, of various forms and colors, at will, almost instantly. He, too, like Miss S., found the complementary color appearing on a white background upon opening the eyes. The cross 'has a tendency' to appear in red of a very intense and brilliant hue (the complementary color was described as a 'very bright silvery blue'). So strong is the effect of gazing at this voluntarily

produced figure that the experimenter feels an almost irresistible tendency to vertigo, and can sometimes scarcely avoid losing consciousness. The colors of the cross are apt to run through the order of red, orange, yellow, green, and blue. They can be arrested by opening the eyes; and after closing the eyes again, they can be made to begin at any point in the series. Mr. D—s thinks the emotional conditions have a great influence on the result. He also finds his eyes in a condition of strain, and the result of fatigue with him is to make the images grow fainter and fainter. His testimony is that he has thus seen crosses of all shapes, sizes, and colors; and apparently he has no difficulty in their construction at will.

I leave this very interesting and, I think, fruitful subject with others to continue the experiment, after noting two or three particulars. One trivial circumstance is that the vertical bar of the cross seems much the easier to produce and to hold steadily in the retinal field. Again, all the experimenters are agreed that the phenomenon is one of *will*, and that the power grows somewhat rapidly with practice. None of them —with the exception, possibly, of Mr. D—s, who speaks of a feeling rather of muscular 'strain'—experience in the production of these images any movement of the eyes. The effect of fatigue seems to vary somewhat; while with most it impairs the result, we have seen that it may operate with one experimenter to make the images disagreeably persistent.

Finally, I venture to affirm—at least in a tentative way —that we have here an experimental demonstration of the unique and inexplicable power of the volition of the ego to induce changes in the cerebral centres and the connected organs of sense,—and in this case, apparently, without any use of the muscular system to control the nature of those changes.

PSYCHOLOGICAL NOTES ON HELEN KELLAR.

BY PROFESSOR JOSEPH JASTROW,

University of Wisconsin.

During the past summer I had the opportunity of making a few tests and observations upon Helen Kellar, the blind and deaf girl whose life and education, in many respects, offer a still more interesting and attractive subject than the remarkable career of Laura Bridgman. These notes are altogether meagre and fragmentary and offer nothing more than indications of the special development of her faculties. I am urged to print them simply because no more thorough study has as yet been undertaken. The tests were made for the most part in the Psychological Laboratory in the Anthropological Building at the World's Fair, Chicago; and for a more complete description of some of the apparatus used I would refer to the Official Catalogue of the Anthropological Building, pp. 50–60.

My first tests related to her powers of touch and movement. For the pressure sense two series of weights, the first increasing by $\frac{1}{15}$, the second by $\frac{1}{30}$, beginning with a standard weight of 300 grammes, were to be arranged in order. Both sets were correctly arranged, and in a rather brief time. As these weights were raised by the hand the main sense involved was the muscle sense; about one third of all persons tested with these weights were able to arrange both sets correctly. The test accordingly indicates nothing more than a muscle sensibility at least normally delicate. Accurate tests would have required more time than was at my disposal.

An æsthesiometer applied to the tip of the forefinger of the left hand indicated that with a distance of 1.5 mm between the points they were clearly felt as double, while points sepa-

rated by 1 mm felt like one broad point. The sensibility was not finer than this on the tip of her tongue. On the palm of the hand points 3 and 4 mm apart were felt as distinct. The normal sensibility for these parts of the skin is differently given by various writers; but if I may trust to the averages obtained from experiments upon a general public, Helen's finger-tips and the palm of her hand (a region interesting because it is here that the impressions of the manual finger-alphabet which she 'reads' are in part received) are decidedly more acute than in the average individual.

In the next test two series of five graded surfaces were presented to her forefinger (right hand), and by passing the finger across the wires she was to obtain a notion of their relative roughness or coarseness, and indicate their order in this respect. The surfaces were produced by tightly wrapping brass wires of various grades around an iron form. In the first series the wires increased in diameter by $\frac{1}{4}$, beginning with a wire .051 inches in diameter, and in the second series they increased by $\frac{1}{8}$. Helen arranged both series in order correctly and with considerable confidence in her judgment. Less than one fourth of all persons tested succeeded in doing this, and there was rarely any confidence in the correctness of the result.

I attempted a more accurate test of the delicacy of the 'form-sense' of her finger-tips by means of the very serviceable touch-apparatus which has been devised by Prof. Münsterberg. In this instrument small wire forms of several sizes and shapes are applied to the skin in order to determine to what extent the form can be distinguished. The best evidences of acute sensibility that I obtained were as follows: a right angle 10 mm on each side was correctly called such and distinguished from an angle of 60°; a set of 8 points set upon a wire circle 10 mm in diameter was at first called 10 points and then 8, set in a 'round'; a series of 10 points set 3 mm apart, with the two central ones separated by 5 mm, was called 'nearly ten,' and some 'not the same distance apart.' A few tests with raised types were made, but with no noteworthy result; the statement of her teacher, Miss Sullivan, that Helen is not a rapid reader is interesting in this respect.

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Furthermore a few observations with tuning-forks were extremely suggestive. The vibrations of a tuning-fork with a pitch of 1024 were distinctly, almost painfully, perceived when the finger-tip was placed lightly on the prong; and the same is true of one with a pitch of 1365, while the vibrations of a fork of about 5000 were not perceived. With the one of 1024, particularly, the vibrations could be felt by the finger when $\frac{1}{2}$ or $\frac{3}{4}$ of an inch away from the fork. This suggests a sensitiveness to the vibration-sense or sense of jar which has frequently been noted by the deaf, and has been well described by Dr. Kitto in his 'Lost Senses.' Further experiments in this direction are desirable. Helen's motor faculties seem not unusually well developed and are doubtless far surpassed by many blind persons. Miss Sullivan has observed that she is not skilful in finding her way about nor in knowing where things are in a familiar room. In the apparatus for testing the accuracy of the perception of lengths by finger-movements, the task is to arrange in order two series of five lengths, the one advancing by $\frac{2}{15}$, the other by $\frac{1}{15}$, from a standard of 150 mm. The first series was correctly arranged, in the second there was one error, and in both there was considerable hesitation and uncertainty. I next arranged a board about 2 ft. square, ruled off in inch squares. A needle was set in a convenient wooden handle, and a thumb-tack was placed at various points upon the board. Helen's finger was first guided to the tack, then taken away, whereupon she attempted to place the needle upon the tack. She was seated with the centre of the board opposite to the centre of the body and moved the needle on an average through a distance of 12 to 15 inches. Her errors in four trials were 35, 15, 15, and 25 mm, a degree of accuracy which may well be equalled by a seeing person with his eyes closed.

The rapidity of her movement seems also below normal. A single test indicated a maximum finger-movement of about 2.5 per second, where the normal (for adults) is about 5 per second. Helen is right-handed, and the attempt to move the two hands simultaneously to an equal extent, away from the centre of the body, indicates the same fact. For left-hand excursions of 133, 138, 169, and 99 mm the right-hand equivalents were 210, 168, 253, and 162 mm. The attempt to draw lines of

equal length, or mark off equal distances by making a series of dots on a strip of paper, showed about a normal degree of error.

Tested with Prof. Cattell's pain-tester she declared a just-perceptible degree of pain when a pressure of 3.75 kilogrammes was brought to bear upon the tip of the forefinger of her left hand (average of three trials.) The usual result for adult women is about 5 kilogrammes, but the variation is large owing to the subjective difficulty of indicating the pain limit.

I attempted also a few tests of the quickness and scope of more complex processes. Beginning with a simple reaction-time, I touched her left hand and required her to respond by touching a key with her right hand. The times, in hundredths of a second, measured by a D'Arsonval chronoscope, were 36, 17, 16, 34, 16, 14, 15, 25. When the functions of the right and left hand were reversed the times were 28, 32, 16, 16, 20, 22. In the first series the two long reactions were clearly due to an awkward manner of closing the key. Omitting these, the first series gives an average time of 17 hundredths of a second which for a child of 14 years is probably a quick reaction. In the next series if I touched her right shoulder she was to press a key with her right hand, if I touched the left shoulder the left-hand key with her left hand, thus involving a distinction of the location of contact and a choice of movements. The times in hundredths of a second were 18, 20, 25, 22, 16, 36, 29, 24, 22, 26, 32, 29, or an average of 25, making a difference of 8 hundredths of a second for the combined distinction and choice. Compared with the average record of persons unused to reacting this is a decidedly creditable record.

My final notes deal with various memory tests, which were performed by the aid of Miss Sullivan, who spelled upon Helen's hand the letters, numbers, or words which I dictated, whereupon Helen would speak vocally the letters, numbers, or words as she remembered them. It should be mentioned that Helen is so entirely accustomed to vocal utterances that this mode of speech seems to have taken the place in her mental habits of her more primitive mode of answering in the finger alphabet. This was shown by her very strong tendency to murmur the words or letters as she interpreted the move-

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ments of Miss Sullivan's fingers. Such motor innervations clearly offered an aid to the memory, and it was with difficulty that she succeeded in repressing this tendency when I requested her to do so. It should be added that her control of the finger alphabet is remarkable. She accepted with great glee my challenge to speak with her fingers Longfellow's "Psalm of Life" as rapidly as possible, and succeeded in forming nearly seven letters in a second throughout the recitation. This is a rapidity sufficient to test the utmost capacity of a sign-reader to keep up with it. Helen had not at the time a set of single signs for the numerals; to convey to her 1, it was necessary to spell o-n-e. She at once learned a set of signs for use in my tests, but the newness of the acquisition clearly acted to the disadvantage of her memory. I shall therefore omit the tests with numerals, which show about a normal memory-span.

Beginning with letters I have the following, in which the columns O are the original series and R the recalled series.

O.	R.	O.	R.	O.	R.	O.	R.	O.	R
b	*b*	*c*	*c*	*k*	*k*	*c*	*c*	*h*	*h*
m	*m*	*z*	*z*	*l*	*l*	*l*	*l*	*a*	*a*
o	*o*	*l*	*l*	*r*	*r*	*o*	*o*	*y*	*y*
s	*s*	*t*	*t*	*p*	*p*	*f*	*f*	*z*	
i		*v*	*v*	*o*	*o*	*k*	*b*	*r*	*r*
k	*k*	*w*	*u**	*t*	*t*	*b*	*r*	*p*	*p*
r	*r*	*k*	*k*	*i*	*i*	*r*	*k*	*k*	*l*
y	*y*	*s*	*s*	*p*	*p*	*y*	*y*	*c*	
b	*b*	*c*	*c*	*a*	*a*	*a*	*b*	*f*	*y*
c	*c*	*b*	*b*	*y*		*c*	*a*	*y*	*f*
v	*v*	*o*	*o*	*c*		*u*	*o*	*g*	*g*
				f			*u*		
				w	*w*				

With less than 10 letters in a set there were rarely any errors, the series being correctly reproduced in *order*. With the above series of 11, 12, and 13 letters there are a few errors. It is interesting to note that the tendency to recall the first

* In this series she was told there was one error, and immediately corrected the *u* by a *w*.

members of the series and the last is as marked in this variety of tactual motor memory as in the auditory or visual.

I also tried nonsense syllables, but these seemed very confusing, six syllables being as many as she could repeat. With monosyllabic words, such as the following, *gate, bell, moon, foot, nest, kite, meal, chair, nail, toy*, she several times succeeded in repeating thirteen words correctly and in order; while with ten or eleven one could count upon a faultless reproduction. A few of these memory-tests were made in the evening; in the morning of the following day Helen was still able to repeat correctly the series of thirteen words she had learned the evening before, but had repeated them to herself a few times during the interval between the two trials. I have collected comparable data for a few hundred individuals, but they have not yet been finally computed. However, upon the basis of a preliminary survey of my material I have no hesitation in pronouncing Helen's verbal memory decidedly above the normal, and particularly when the correctness of the order is taken into account. How far this may be due to the concentration of her attention upon one sense, and to her acquiring through verbal means what to us is visible or audible, is an open question. The account of her mental habits given by Miss Sullivan (Helen Kellar: The Volta Bureau, Washington, 1892) amply corroborates the extraordinary powers of her literary memory.

I cannot conclude these notes without commenting upon the remarkable alertness and receptivity of mind displayed by her in visiting the exhibits at the World's Fair. By the courtesy of the officials the universal admonition 'Do not touch' was disregarded in her case; and it certainly was most interesting to observe the rapidly-varying expressions of her animated features, and listen to her comments, as one specimen after another from the ethnological collections was placed in her hands with some brief description of its character communicated to her through Miss Sullivan. The acuteness of intellect, breadth of interest, wholesomeness of emotional sensibility, along with such confined avenues of intercourse with the outer world, could not but impress the psychological observer as an admirable illustration of the relative functions

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of the senses, and the faculties that interpret and assimilate the facts of sensation in the economy of the mental life.

My obligations are due not only to Helen Kellar herself for her cheerful compliance with my somewhat arduous demands, but to her able teacher Miss Sullivan, and to the distinguished scientist who has so generously espoused her cause, Dr. Alexander Graham Bell.

PSYCHOLOGY PAST AND PRESENT.*

BY PROFESSOR J. MARK BALDWIN,

Princeton University.

I. HISTORICAL.

Modern psychology has had its principal development in Great Britain, Germany, and France. Germany has undoubtedly had greatest influence in this movement, considered in all its branches. The two main currents of development previous to the rise of the new so-called 'scientific' psychology, designated as 'speculative' and 'empirical,' had their initial impulse, as well as their fruitful pursuit, respectively in Germany and Britain. German psychology down to the rise of the Herbartian movement was a chapter of deductions from speculative principles; English psychology was a detailed analysis of the experiences of the individual consciousness. Kant, Fichte, and Hegel may sufficiently represent the succession in Germany; James Mill, John Stuart Mill, Hume, Reid, and Bain, that in Great Britain.

The work of Herbart and his school tended to bring a more empirical treatment into German thought, and its significance was twofold: it excited opposition to the speculative method, and it prepared the Germans for the results of English analysis. It is further a legitimate supposition that the spirit of experimental inquiry which has swept over Germany in this century was made more easily assimilable by workers in this department, also, by the patient and extraordinary attempt of Herbart to construct a 'mechanic' and 'static' of mind in his 'Psychologie als Wissenschaft' (1824).

* Part of the material to be used in different form in a 'Historical and Educational Report,' prepared by the author (by request) in his capacity as 'Judge of Award' for this subject at the World's Columbian Exposition.

To German thinkers also belongs the credit due to originators of all new movements which show their vitality by growth and reproduction, in that the experimental treatment of the mind was first advocated and initiated in Germany. But of this I write more fully below.

The contribution of France to psychology has been decidedly of less importance; yet the work of its writers has also illustrated a fruitful and productive movement. It has been from the side of medicine that French work has influenced current wide-spread conceptions of consciousness. Mental pathology and the lessons of it for the theory of the mind have come possibly most of all from France; or at any rate—not to disparage the admirable recent work of English and German investigators—the tendency, so to speak, of the French treatment of consciousness has been to approach mental operations from the abnormal side.

In America the influences which have tended to control psychological opinion have been mainly theological on one side and educational on the other. The absence of great native systems of speculative thought has prevented at once the rationalistic invasions into theology which characterized the German development, and the attempts at psychological interpretation which furnished a supposed basis of fact to the idealistic systems. In Germany various 'philosophies of nature' sought to find even in objective science support for theoretical world-dialectic: and psychology fared even worse, since it is, *par excellence*, the theatre for the exploitation of universal hypotheses. But in America men did not speculate much: and those who did were theologians. So naturally the psychologists were theologians also. Jonathan Edwards had a doctrine of the agent because free-will was a question of theology.

The educational influence was auxiliary merely to the theological. The absence of large universities with chairs for research; the nature of the educational foundations which did exist under denominational control; the aim of education as conceived in the centres where the necessity for supplying growing towns with pastors was urgent; the wholesome fact for our civilization that the Puritans had traditions in favor

of the school and the religious school—all these things made it only necessary that books sound in their theological bearings, or affording homiletic lessons in living, should be written, in a topic of such central importance. Even the term 'psychology' is only now getting domesticated: 'mental' and 'moral' philosophy were the titles of courses of instruction on the 'soul.'

The type of philosophy which these conditions encouraged was, it may easily be imagined, realistic; and it is probably for the reasons which I have indicated that the Scottish Natural Realism was the American type of thought, and is now, except in the great university centres where systematic philosophy has become an end in itself apart from its duty to theology and education. As far as psychology was concerned, this realistic tendency was a great good. It led to a magnification of mental reality, to a reverence for the 'utterances of consciousness,' to a realistic interpretation of the 'immediate knowledge of self,' to the firm settling of the great 'intuitions,' cause, time, space, God, etc.; and in as far as this led to the direct examination of consciousness and to the testing of philosophical claims by consciousness, it prepared the way for a better and broader method. This tendency is marked even in the more influential works in theology. Channing and Emerson no less than Smith and Charles Hodge lay the corner-stone of argument again and again in the proof 'from consciousness.'

This tendency to a psychological view of philosophy and its basis in the religious motive is seen also in Scotland, the home of realism: and it is there a part of the British method of thought which I have already spoken of. The works on psychology written in America up to 1880 were, as we should expect, from the hands of theologians and educators, usually both in the same person; for it is a further proof of the association of psychology and theology that the mental and moral philosophy in the colleges was almost without exception put in the hands of the president of the college, and he was by unanimous requirement a preacher. So were written a series of works which are landmarks of American scholarship, props of evangelical theology, disciplinary aids of the highest value

to the growing student, and evidences—to revert again to my argument—of the twofold influence I have indicated. Edwards's 'Freedom of the Will' (1754), Tappan's 'Review of Edwards' (1839) and 'Doctrine of the Will determined by an Appeal to Consciousness' (1840), Hickok's 'Rational Psychology' (1848) and 'Empirical Psychology' (1854), Porter's 'Human Intellect' (1868) and 'Moral Science' (1885), McCosh's 'Psychology' (1887) and 'First and Fundamental Truths' (1889)—these and other books like them show the psychology of America up to about 1880.

Speaking for psychology alone, not for philosophy, it is easy to point out their merits and defects, not in my individual judgment, but as compared with the standards of the present year of the Exposition. It is necessary, however, rather to show this by sketching the present and showing the new elements which have modified the American work and whence they came.

Coming to the present state of psychological thought, my task is made easier by reason of the divorce which has been forced between psychology as a science on one hand and metaphysics on the other. As was said above, Herbart, while failing in his attempt to apply mathematics to mental 'permutations and combinations,' yet prepared the way for a new treatment of mental phenomena. After his attempt it began to be seen that the facts of conscious life were first in order of importance and were capable of treatment in a detailed way quite independently of the questions of Being, the Absolute, and the like. The works of Volkmann, 'Lehrbuch der Psychologie' (4th ed., 1894), and Lipps, 'Die Grundthatsachen des Seelenlebens' (1883), illustrate this.

This was only to begin to do what had been doing in England since Locke. But the Germans now went further: they asked the question—which had been groped upon before by Descartes, by Leibnitz, and by Reid—how can psychology be a science when one of the evident conditions of the flow of mental states, of their integrity and their trustworthiness, the brain, is left quite out of account? What is the law of connection of mind and brain? And is it possible to modify the brain and so to modify the mind? If so, then that great in-

strument of scientific work, experiment, may perform a part for the psychologist also, and his resources be magnificently enlarged.

This is the question of Experimental Psychology. It was answered in Germany in the affirmative. Lotze, in my view, deserves the credit of it, the credit of the great-minded constructive pioneer; and Wundt is the founder of the science in the sense that he first realized the expectations of Lotze's genius by actually planning and executing experiments on a large scale which made the affirmative answer an irreversible fact of history. Lotze's 'Medicinische Psychologie' appeared in 1852, Wundt's 'Grundzüge der Physiologischen Psychologie' in 1874. Between the two, however, came Fechner, whose theoretical construction of the new work and its methods shows all the exactness of treatment of similar discussions of natural-science principles by electricians and chemists, and published the formulas in which he attempted to give universal statement to the discoveries of E. H. Weber on the intensity of sensation-states. Fechner's 'Elemente der Psychophysik' appeared in 1860.

Apart from the actual development of this new method—a point to be spoken of later on—it has profoundly modified the general conception of psychology even where its validity as a method has been denied. There has been nothing less than a revolution in the conception of psychology since the publication of the works just named. One of the motives of this revolution came thus from Germany. The other—for it has two great phases—is due to English thinkers: the evolutionists, of whom Herbert Spencer ('Principles of Psychology,' 1855) is the chief. These two influences are seen in two great points of contrast easily made out between the psychology of to-day and that of yesterday in America. The latter I have described above. Its two main characteristics, for purposes of the present contrast, are first, its character as so-called 'faculty-psychology'; and second, its character as holding to what I may call a 'ready-made' view of consciousness—technically an 'intuition' view of consciousness. In opposition to these characters, current psychology is 'functional'—holding to mental 'functions' rather than to mental faculties; and finds this

function to be 'genetic' rather than intuitive — the functions 'grow,' instead of being 'ready-made.'

The old conception of 'faculties' made the different phases of mental process in large measure distinct from one another. Memory was a 'faculty,' a 'power' of the mind; thought was another, imagination a third. The new functional conception asks how the mind as a whole acts, and how this one form of activity adapts itself to the different elements of material which it finds available. The old terms 'memory', 'thought,' etc., are retained; but with the distinct understanding that they do not stand for divisions in the mind, or different processes, one of which may be held in reserve when another is acting, etc. On the contrary, the process in consciousness is one; and it is a psycho-physical process as well. The particular way in which this one function shows itself is a matter of adaptation to the changing conditions under which the activity is brought about. This transition is due in part also to the insight of Herbart and to the demand for unity insisted upon by the evolutionists.

The other point of contrast is equally plain. The 'genetic' point of view in current discussion is opposed to the older 'intuitive' point of view. The mind is looked upon as having grown to be what it is, both as respects the growth of the man from the child, and as respects the place of man in the scale of conscious existences. The understanding of mental facts is sought in the comprehension of their origin as well as their nature: and the question of the validity or worth of 'intuitive' beliefs in consciousness is subordinated to the question as to how the mind came to have such beliefs.

Both of these points of contrast have been further defined by the progress of general philosophy in America. The demand for unity in mental interpretation has not come from naturalistic evolution alone (John Fiske, 'Outlines of Cosmic Philosophy,' 1874; Thompson, 'System of Psychology,' 1884); an equally pressing demand has come from idealistic metaphysics, which seeks for continuity in the natural series as zealously as does the advocate of evolution. The influence of Hegel, as interpreted in the works of Green, and later in those of Caird, has been potent in effecting this transformation. It

is easy to see also that the same union of forces is quite feasible as respects the genetic development of consciousness, although the new idealists have not done justice to this growing tendency in modern psychology.

The line of cleavage, in the current discussions of general psychology, is drawn on the question of the interpretation of mental 'function': both sides claiming the same full liberty of genetic research and the same resources of analysis and experiment. The 'Associationists,' on one hand, carrying on the tradition of the British empiricists, construe mental function after analogy with the ordinary interplay of forces in the objective world; the 'Apperceptionists,' on the other hand, hold that mental function is a form of irreducible cosmic process. Apart from original monographs on special topics, no work on psychology to-day commands much attention, either from psychologists or from students of philosophy, which does not show itself alive to this main issue. The works of Lotze and Wundt have had great influence upon Americans in the direction of this general statement of the problems of psychology: and it is especially the philosophy of Lotze which is replacing by a reasoned and critical realism the earlier theological dogmatic view so long prevalent in the United States by inheritance from Scotland.

On the literature of present-day psychology I can do no better than quote the following passage freely translated from the most recent German work on general psychology, itself fully representative of the present state of knowledge—'Grundriss der Psychologie,' by Professor Külpe of the University of Leipzig (pp. 27 ff.):

"About the middle of the nineteenth century experimental and psycho-physical psychology began its course in Germany. While Herbart recognized a threefold influence of the body upon the mind, . . . it was Lotze who made a thorough beginning in the employment of the data of physiology. Lotze, indeed, began his work with certain metaphysical expositions after the manner of the older German writers, and is very far from the recognition of a universal psycho-physical parallelism. But he does not hesitate to speak of the nervous conditions of mental processes, and he had the good fortune to suggest

hypotheses of value where exact knowledge was wanting. The real foundation of Experimental Psychology was laid, however, by G. T. Fechner, who sought to carry out in a thorough-going way the conception of a functional relation between mental and physical processes. Although the mathematical form which he gave to this relation . . . does not hold, yet he gave to the exact science of psychology an extraordinary impulse, by reason of the new conceptions which he introduced, the methods of procedure which he both formulated and applied, the working over which he gave to the material he had in hand, and the observations and researches which he himself carried out. . . . The union of the experimental and psycho-physical was finally accomplished by Wilhelm Wundt . . . in his classical 'Grundzüge der Physiologischen Psychologie' (1874, 4th ed. 1893). By this unity of conception and by his comprehensive treatment of all mental phenomena . . . he has made the current phrase 'modern psychology' applicable. . . . Wundt gave a further important impulse to the cultivation of experimental psychology by founding the laboratory in Leipzig in 1879, and establishing the 'Philosophische Studien,' a journal devoted mainly to the publication of researches from his institute.

"Additional works may be mentioned of very recent date, which must be reckoned in their character as belonging to the modern psychology thus founded by Wundt, although they differ more or less essentially in system and in theory from him and from one another: Höffding, 'Psychologie in Umrissen,' 2d ed., 1893, German translation from the Danish (English translation, 1891); Ladd, 'Elements of Physiological Psychology,' 1887; Sergi, 'La Psychologie Physiologique' (translation from the Italian, 1888); W. James, 'The Principles of Psychology,' 1890; Ziehen, 'Leitfaden der physiologischen Psychologie' (1891; 2d ed., 1893); Baldwin, 'Handbook of Psychology,' 1891 (2d ed.; 1st ed., 1889–90); J. Sully, 'The Human Mind,' 1892.

"We may mention also certain periodicals which represent the same current of psychological thought: 'Philosophische Studien,' edited by W. Wundt (vols. 1–8, 1883 ff.); 'The American Journal of Psychology,' edited by G. S. Hall (vols.

1–5, 1887 ff.); 'Zeitschrift für Psychologie und Physiologie der Sinnesorgane,' edited by H. Ebbinghaus and A. König (vols. 1–5, 1890 ff.)."

The part taken by American students in the present psychological movement is seen in the fact that of the seven works thus cited by Külpe three are by Americans, and to them must be added 'Psychology: Descriptive and Explanatory' (1894), by G. T. Ladd, and the journal 'The Psychological Review,' edited by J. McK. Cattell and J. Mark Baldwin (vol. 1, 1894). Another important French work of recent date is 'La Psychologie des Idées-Forces,' by A. Fouillée (1893). The position of psychology in the American colleges and universities is described in a further section below.

Other important contributions to Experimental Psychology —apart from the long series of monographs and research articles published in Germany and America—are Helmholtz, 'Physiologische Optik' (1867, 2d ed. f. 1886, French translation), and 'Tonempfindungen (1863, English translation); Stumpf, 'Tonpsychologie' (1883–90); and Münsterberg, 'Beiträge zur experimentellen Psychologie,' Parts I–IV (1889–93).

The contribution from the side of mental pathology has become important on account of the *rapprochement* which has obtained in recent years between the alienist and the psychologist. The works of Pierre Janet, 'Automatisme psychologique' (1889) and 'L'État mental des Hystériques' (1892–93), and of Bernheim, 'Suggestive Therapeutics' (English translation, 1889), and 'Études de la Suggestion' (1892), are most important. To them should be added the works of Ribot, 'Diseases of the Will,' English translation (5th French ed., 1888); 'Diseases of Memory,' English translation (5th French ed., 1888); 'Diseases of Personality' (2d ed., 1888; English translation, 1891), together with the many original contributions on the subject of hypnotism and aberrations of personality published in the 'Revue Philosophique' (edited by Th. Ribot, vols. I–XXXVI, 1876 ff.) and summed up in part in 'Les Altérations de la Personalité (1893) of Alf. Binet.

Further, the treatment of psychology in accordance with the British tradition, from the point of view of description and analysis, has been carried forward by Ward in the article

'Psychology' in the Encyclopædia Britannica, 9th ed. This type of research has also had its organ of publication in 'Mind: a Journal of Psychology and Philosophy,' edited by G. Croom Robertson (vols. I–XVI, 1876 ff.) and by G. F. Stout (New Series, vols. I–III, 1892 ff.).

Finally, the genetic treatment of consciousness has been advanced by the works of Spencer, 'Principles of Psychology,' 1855 (3d ed., 1880); Romanes, 'The Origin of Human Faculty,' 1884–1888; Morgan, 'Animal Life and Intelligence' (1891); and Galton, 'Inquiries into Human Faculty' (1883) and 'Natural Inheritance' (1889).

II. The Method and Main Divisions of Experimental Psychology.

To say that this is the age of science is only to repeat what is now trite and what no student either of philosophy or of history needs to be told. It is the age of science because it is the age of devotion to science and of results in science. But it is a very different thing to say that this is the age of scientific method. Former ages have seen devotion to science and results in science, but I venture to say that no former age has, as an age, realized a scientific method. So prevailing, however, has the new method now become, and so customary to us, that it is only by historical study that we are able either to see that it is new, or to work ourselves into that degree of intellectual sympathy for the old which the earnest endeavor and unflagging patience of the heroes of philosophy in the past rightfully demand for all time.

In characterizing our time by the word 'scientific,' as regards method, I mean to say something which is true in philosophy, politics, literature, as well as in the investigation of nature; and to dwell only on the department of thought in which such a method has been, and is, most difficult to realize. In philosophy it is not fully realized; and yet I believe that any class or school of philosophic thinkers who do not face toward the scientific east are steering up-current and will be absent when science and philosophy enter a common barge and together compass the universe of knowledge. For it is a

part of the same conviction as to scientific method that neither science nor philosophy will ever succeed in compassing it alone. However painfully this advance may have been won and however loudly the dogmatists may deny its justification, it is sufficient here to signalize the fact that philosophy has in the present half-century thrown open her doors to the entrance of critical and empirical methods, and that the results already accruing are evidence of the bigness of her future harvest.

In general philosophy what has been called scientific method is better known, as I have said above, in a twofold way, as empirical and critical. Retrospectively what we now have to rejoice in in philosophy is due about equally to two traditions, represented by Hume and Kant. The burden of current idealism, as far as it is worthy of consideration in our time, is to purify and conserve the work of Kant. And the burden of empiricism, under the same restriction, is to refute Kant with the only weapons which he himself considered of worthy temper. The battle is drawn at these close quarters, and round them both is thrown a common ring of scientific procedure.

In psychology the modern transformation comes most strongly out. Here we find an actual department of knowledge handed over to a new class of men for treatment, so remarkable is the demand for scientific method. It is no longer sufficient that a psychologist should be familiar with general philosophy and its history, or capable of acute logical criticism of systems; it is necessary, if he would deal successfully with the new problems and gain the ear of the advanced philosophical public, that he should reason from a basis of fact and by an inductive procedure. In short, he must not bring his philosophy as speculation into psychology, but must carry his psychology as fact, in its connection with physiology, ethnology, etc., into general philosophy.

To illustrate this change, and its effect on general theories, recent discussions of the idea of space may be cited in comparison with its earlier and more speculative treatment. The reasonings of James, Wundt, Bain, Spencer, differ so essentially from the argumentation of Kant and earlier men that it is almost impossible to find common ground between them. No one among those who accept Kant's results depends in our

day very largely upon his reasons: the question is shifted to another field. The physiologist has as much to say about it to-day as the psychologist, and the speculative philosopher must recognize them both.

This tendency of the day in philosophy may be expressed by a chemical figure as a 'precipitating' tendency. We are endeavoring, and successfully too, to throw all questions which are capable of such treatment to the bottom, as a precipitate—a psychological precipitate—and are then handing them over to the psychologist for positive treatment. As long as our data remained in a solution of ninety parts water (which, being interpreted, means speculation), it was difficult to handle them scientifically. While admitting the utility and necessity of ontology in its place, current psychology claims that its place must be better defined than formerly it has been, and that whenever we can secure a sediment, a residuum, a deposit, apart from a speculative solvent, this is so much gain to positive science and to truth.

One of the ideas which lie at the bottom of the so-called 'new psychology' is the idea of measurement. Measurement, determination in quantity and time, is the resource of all developed science, and as long as such a resource was denied to the psychologist he was called a scientist only in his function of description and classification, not in the more important functions of explanation and construction. And the justification of the application of measurement to psychological facts has come, not from theoretical considerations—for they were all opposed, and still are, in many of the books of the new idealism—but from practical attempts to do what philosophy declared to be impossible. That is, experiment has been the desired and only 'reagent.' It is true that theoretical justifications are now forthcoming of the application of experiment to consciousness, but they are suggested by the actual results and were not in sufficient currency to hinder the influence of Kant's ultimatum, for example, that a science of psychology was impossible.

By experiment in this connection is meant experiment on the nervous system with the accompanying modifications it occasions in consciousness. Efforts have been made in earlier

times to experiment upon states of consciousness directly. Descartes deserves credit for such efforts, and for the intimation he gives us, in his theory of emotions, of an approach to mind through the body. But the elevation of such an approach to the place of a recognized psychological method was not possible to Descartes, Kant, or any one else who lived and theorized before the remarkable advance made in this half-century in the physiology of the nervous system. And even as it is, many questions which will in the end admit of investigation from the side of the organism are still in abeyance till new light is cast upon obscure processes of the brain and nerves.

A little further reflection will show us that the employment of experiment in this sphere proceeds upon two assumptions which are now generally admitted and are justified as empirical principles, at least by the results. They are both assumptions which the physical scientist is accustomed to make in dealing with his material, and their statement is sufficient to exhibit their elementary importance, however novel they may sound to those who are accustomed to think and speak of mind as something given to us in entire independence of organic processes. The first of these assumptions is this: that our mental life is always and everywhere accompanied by a process of nervous change. This is seen to be necessary to any method which involves the passage of mind to body or the reverse by the interpretation of effects. Which is cause and which effect, the mental or the physical change, or whether they both are effects of an unknown cause, is immaterial—to consider such a question would be to introduce what I have called the 'speculative solvent.' It is sufficient to know that they are always together, and that the change in one may be indicated in symbols which also represent the change in the other. The second assumption is based upon the first, viz., that this connection between mind and body is uniform. By this is meant what in general induction is called the uniformity of nature. Any relation sufficiently stable to admit of repeated experiment in the manipulation of its terms is in so far uniform. Experiment would be useless if the relation it tends to establish were not stable, since the result of such experiment would give no antecedent likelihood as to the result of others under similar

circumstances. Experimental psychology, therefore, rests upon the assumption that a relation of correspondence—be it coexistence or causation—once clearly made out between a mental and a nervous modification, it must hold good under any and every repetition of the same experiment under the same conditions.

These two assumptions made, we have at once the possibility of a physical approach to the facts of consciousness. The result is a relative measurement of such facts in terms of the external stimulation of the nerves, in regular and normal conditions of the activity of attention.

Further, it is apparent that such a means of experimentation may become available either under artificial or under natural conditions, according as the nervous stimulation is due to an external excitation, or arises from some unusual condition of the organism itself. All cases of brain or nervous disease, on the one hand, offer opportunities for boundless observation; the unusual manifestations being changes due to the organic disturbances of disease. Here nature has arranged and actually performed the experiment for us; the only difficulty being the physiological one, that the cerebral states may be as obscure as the mental states which they are used to explain. All such cases of mental changes due to internal organic changes are classed together under the name of Physiological Psychology. It includes all questions which relate to nerve physiology and pathology, illusion, hallucination, mental disease, hypnotism.

On the other hand, experiments may be arranged for the normal stimulation of the sense-organs—skin, muscles, special senses—under artificial conditions as explained in part below. This is Experimental Psychology. On these lines modern experimental psychology falls into two great departments. As the normal properly precedes the abnormal, it is well to consider the line of researches based upon external experiment, confining ourselves to a more or less cursory view of results of historical interest.*

*In the official report, sections are included on 'Psycho-physics' (Weber's Law) and 'Mental Chronometry' (Reaction-times).

III. The Exhibits in Psychology at Chicago.

We are now prepared to consider the exhibits made in the interests of Experimental Psychology at the Columbian Exposition. It is evident that departments in which progress is in the main abstract and immaterial—such as the social, moral, and theoretical sciences—cannot show their work to the eye, and so have heretofore appeared at the world's great expositions only as their results have been embodied in more practical life, in education, and in institutions. It is, however, unfortunate that this should be so: for the more ideal and spiritual aspects of a nation's life are just the aspects in which popular instruction is defective, and these are the aspects which should least of all be omitted in a survey of the conditions of present-day civilization. Yet it is so; and it becomes easy to see, therefore, that it is only as psychology has become experimental and so has found it possible to state her problems and results to a degree in forms which allow of diagrammatic and material representation that she is able to 'exhibit' herself. What psychology showed, therefore, at the Chicago Exposition was the experimental side, as I have sketched its problems and methods in what precedes.

The exhibits bearing on psychology in its scientific aspects —as apart from the educational aspects, of which I speak later on—may be placed in order thus:

(A) A collected exhibit made by the department of Anthropology, of which Professor F. W. Putnam of Harvard University was chief, under the immediate direction of Professor Joseph Jastrow of the University of Wisconsin, consisting of a Psychological Laboratory in operation with all its accessories.

(B) A collection of instruments shown in the German Educational Exhibit under the heading 'Psychophysics.'

(C) Instruments shown in the general exhibit of the 'Deutsche Gesellschaft für Mechanik und Optik.'

(D) The private exhibits of particular instrument-makers.

(E) Exhibits made by single universities, i.e., those by the University of Pennsylvania and the University of Illinois.

I may consider these briefly in order.

(A) *The Laboratory for Experimental Psychology, gathered by the Department of Anthropology (Ethnology).*—This laboratory constitutes the first attempt ever made to exhibit at an international fair the state of progress of the world in this branch. When taken in connection with the other laboratories exhibited by this department, i.e., in Anthropology and Neurology, it may be accepted, in its main features, as an adequate historical index of the psychological progress of the nineteenth century. The general features of the working laboratory cannot be better described than in the words of the director, Professor Joseph Jastrow.*

The Psychological Laboratory.—"The object of this laboratory is to illustrate the methods of testing the range, accuracy, and nature of the more elementary mental powers, and to collect material for the further study of the factors that influence the development of these powers, their normal and abnormal distribution, and their correlation with one another. The laboratory is thus designed, not as are those connected with universities, for special research, or for demonstrations and instruction in psychology, but as a laboratory for the collection of tests. As in physical anthropometry the chief proportions of the human body are systematically measured, so in mental anthropometry the fundamental modes of action upon which mental life is conditioned are subjected to a careful examination. In both cases the first object is to ascertain the normal distribution of the quality measured. With this determined, each individual can find his place upon the chart or curve for each form of test and from a series of such comparisons obtain a significant estimate of his proficiencies and deficiencies. It should not be overlooked that mental tests of this kind are burdened with difficulties from which physical measurements are comparatively free. Our mental powers are subject to many variations and fluctuations. The novelty of the test often distracts from the best exercise of the faculty tested, so that a very brief period of practice might produce a more constant and significant result. Fatigue and one's physical condition are also important causes of variation. It is im-

* Official Catalogue of Exhibits, Department M, in which full descriptions may be found.

possible in the environment of the present laboratory to secure the necessary time and facilities for minimizing these objections. They detract more from the value of an individual record than from that of the combined statistical result. So much remains to be done in this line of investigation that at every step interesting problems are left unanswered. But what has been done emphasizes the importance and probable value of further research. The problems to be considered when once the normal capacity has been ascertained are such general ones as the growth and development with age of various powers; what types of faculty develop earlier and what later; how far their growth is conditioned upon age and how far upon education; again, the difference between the sexes at various ages, differences of race, environment, social status, are likewise to be determined. The relation of physical development to mental, the correlation of one form of mental faculty with others, the effect of a special training, —these, together with their many practical applications, form the more conspicuous problems to the elucidation of which such tests as are here taken will contribute. In addition to the interest in his or her own record, the individual has thus the satisfaction of contributing to a general statistical result."

(B), (C), (D), (E) *The Exhibits of* (B) *the German Educational Department*, (C) *the 'Deutsche Gesellschaft für Mechanik und Optik*,' (D) *Individual Private Instrument-makers, and* (E) *Separate Universities*.—The two German agencies mentioned as (B) and (C) send what may be considered as on the whole the best indication—when taken in connection with the special pieces of apparatus sent from German workshops to the collective exhibit of the department of Anthropology—of the application of modern mechanical skill to the construction of instruments of the delicacy required for psychological experiment. These instruments are mainly adaptations of well-known principles, and often of well-known apparatus, used in experimental physiology, physical optics and acoustics, electricity, etc. The instruments shown by the German Mechanical and Optical Society are almost entirely common to psychology and these sciences. The pieces in the German Educational Exhibit are largely the special arrangements found useful in the labora-

tory at Leipzig, and so show very inadequately the real progress of the science in Germany. Yet they are of great historical interest. The collection is much less complete than that made by the German instrument-makers in connection with the collective exhibit in the Department of Anthropology. In this connection it should be mentioned that the account given of Experimental Psychology in Germany by Professor Wundt in the official book, 'Die deutschen Universitäten' (ed. by W. Lexis, 1893), is not adequate if considered (and probably the author did not intend it to be so considered) as an exponent of the present condition of this science and the place it occupies in the German universities.

(D) The private exhibits of individual firms should be noted in the attempt to make one's conception of psychological activity complete. French exhibitors did not combine as the Germans did, and so lost both in effect and in local position. Yet much of the finest work is done in Paris, as is witnessed by the cases of surgical, physical, and psychological instruments grouped in the north end of the Anthropology building. An examination of the catalogues of the exhibitors (for example, that of Ch. Verdin of Paris) may serve for the details of this class of exhibits, as the united catalogues of the other collections mentioned serve in respect to them. The German makers have done their work more largely in connection with great university laboratories, and so have subserved better the needs of particular students in solving particular problems in physics and psychology: the French, on the other hand, have found the demand more marked from the side of clinical medicine and experimental physiology.

(E) The separate university exhibits of the Universities of Pennsylvania and Illinois were located respectively in the Liberal Arts and the Illinois State building. The aim of the former was to present a working laboratory restricted to a small number of topics. This original purpose was not subserved through the failure to provide attendants to collect experimental data; yet the arrangements for experiments in reaction-times and the visual æsthetics of form were instructive to visitors. Two pieces of new apparatus were exhibited by Dr. Lightner Witmer, the designer of them: a complex

color-wheel which permits the alteration while in motion of the proportion of colors mixed, and a graphic movement apparatus involving new features.

The exhibit of the University of Illinois was mainly of instruments which were also included in the main collection of the Department of Ethnology. It was in charge of Professor W. O. Krohn of that university.

IV. Educational.

The educational aspects of the new work in psychology are of great importance. It is evident that education has two claims to make upon this study; one of these claims the old psychology aimed to meet, the other it was incapable of meeting. The first of these two duties of psychology to education is this: it should take its place as a factor in liberal collegiate culture in both of the functions which a great branch of learning serves in the university curriculum, i.e., undergraduate discipline and instruction, and post-graduate research discipline.

The older psychology, especially in America where it was hampered by the conditions pointed out in an earlier section, did, as I say, aim to instruct undergraduates. But even in this it was a means to another end: it was propædeutic to a philosophy and to a theology, both of which, as far as their demands upon 'mental science' were concerned, were dogmatic and intolerant. But the graduate disciplinary function was never served in any sense by the faculty psychology nor by the philosophy founded upon it in America.

The second great educative function of psychology is this: it should mould and inform educational theory by affording a view of mind and body in their united growth and mutual dependence. Education is a process of the development under most favorable conditions of full personality, and psychology is the science which aims to determine the nature of such personality in its varied stages of growth, and the conditions under which its full development may be most healthfully and sturdily nourished. One of the first duties of psychology, therefore, is to criticise systems of education, to point out 'the

better way' in education everywhere, and to take no rest until the better way is everywhere adopted. This duty the old psychology did not realize: indeed, by its method and results it was cut off from the realization of it. It shall now be my aim to show how contemporary psychology is addressing herself to all these undertakings.

A. Psychology as Research Discipline.—I begin with this point because it is the most striking fact about the present state of psychology in all countries where the experimental idea has been given entertainment. Probably students and general readers hear more about 'research' in connection with psychology than with any other branch. And it is odd —indeed to workers in other departments amusing—that all this claim to research ability, and talk about 'original contributions to knowledge,' is by professors who are yet smooth-faced and generally quite inexperienced in university affairs. A physicist who makes contributions to knowledge is extremely rare, but the 'new psychology' has two men of research to every competent college instructor in its ranks.

This, I take it, is a hopeful and encouraging state of things, and has its origin in two influences: first, the new impulse has come from Germany, where the university-function corresponds very nearly to the graduate-discipline function in the few American institutions where graduate work is encouraged; and second, because the actual state of the subject is such that research is a matter of comparatively less difficulty than in the older scientific branches. Yet the actual value of this condition of things in the permanent development of the subject must be held to be disciplinary and educational; for the more serious and philosophical of the psychologists do not expect these first results of the new methods to be revolutionary in their value, nor have the researches so far published been much more than suggestions of what may be done when the method is held under better control and those who apply it have had adequate discipline and training in its use.

Accordingly, in my view, the very marked tendency to 'research' evident in the management of the new laboratory foundations of the colleges in this country is of main value as offering training to the future instructors in psychology

throughout the land, rather than as offering contributions to knowledge. The students in these laboratories come largely from colleges where experimental psychology is unprovided for or held up for criticism by professors of philosophy. The utilization of their results, except in problems whose solution properly involves ignorance, crudity, and liability to individual variation, is manifestly impossible.

The research discipline offered by graduate work is indispensable, however, as discipline, since it is at present the only substitute for undergraduate discipline. This, indeed, is the function of graduate work in the other departments of science in the universities. It is emphasized, however, in psychology since, as I shall show below, undergraduate instruction in experimental psychology is still in an inchoate condition even in the few larger institutions in which it has been added to the B.A. course of study.

Chairs in Experimental Psychology occupied by men whose principal function is graduate discipline—although in some institutions the undergraduate function is being recognized—are now no longer novelties. Abroad the German universities take the lead in such instruction; yet the instructors are generally professors of philosophy or of psychology who offer experimental courses. Laboratory foundations began in Germany in 1878 with the Institute at Leipzig (Professor Wundt); they are now to be found as well at Berlin (Professor Ebbinghaus, now at Breslau), Göttingen (Professor Müller), Bonn (Professor Martius), Prague (Professor Hering), Munich (Professor Stumpf, now at Berlin), and Heidelberg (Professor Kräpelin). As for other European countries, a chair of Experimental Psychology was founded at Paris in the Collège de France in 1886 (Professor Ribot), and a 'Laboratoire de Psychologie physiologique' opened in the Sorbonne in connection with the École des Hautes-Études in 1891 (Professors Beaunis and Binet). Other such Continental foundations are to be found at Geneva (Professor Flournoy) and at Rome (Professor Sergi). At Florence a laboratory and museum of Psychology and Criminal Anthropology has recently been instituted (Professor Mantagazza). In Great Britain and her possessions the analytic method has not given way to the experimental. In Canada

alone, at the University of Toronto (Professor Baldwin, now Dr. Kirschmann), a well-equipped laboratory was opened in 1891, although a little later a small sum was secured for the purpose of beginning work of this kind at the University of Cambridge, England. Lectures are given, however, both by physiologists (Professor Hill at University College, London, 1894) and by professed psychologists (Professor Alexander, Owens College, Manchester, 1893). Japan follows with one such laboratory—that at the University of Tokio (Professor Motora).

In the United States the extension of this method of treatment has been rapid, and the establishment of chairs and of laboratories extraordinary. The first laboratory was established in 1883 at Johns Hopkins University (Professor Hall), but it has since been closed. This was followed in 1888 by the establishment at the University of Pennsylvania of the first chair of Psychology alone with a laboratory (Professor Cattell). Here the first undergraduate laboratory instruction was given. Later, chairs for Experimental Psychology alone have been erected at Columbia College (Professor Cattell), Harvard University (Professor Münsterberg), where an additional Professorship in General Psychology exists side by side with it (Professor James), the College of New Jersey at Princeton (Professor Baldwin). Professorships either in Psychology as a whole, or as associated with Education, exist at Clark University (Professors Hall and Sanford), Wisconsin University (Professor Jastrow), Cornell (Professor Titchener), Chicago (Professor Strong), Indiana (Professor Bryan), Illinois (Professor Krohn), Stanford (Professor Angell), Catholic University at Washington (Professor Pace), Wellesley College (Miss Professor Calkins). At all these institutions laboratories with equipment have been provided; and such provision has been made in others where no separate professorships have yet been erected, i.e., Yale (Professor Ladd), Brown (Professor Delabarre), Minnesota (Professor Hough), Nebraska (Professor Wolfe), Michigan (Professor Dewey, now of Chicago).

The nature of these laboratories is illustrated by the large exhibit already spoken of. That at Harvard University is the largest, best equipped, and most freely patronized by graduate

students. A Harvard pamphlet-catalogue of the apparatus in the laboratory, containing also illustrations, bibliographies, and a list of topics under investigation (23 in number), was prepared by Professor Münsterberg for the collective university exhibit. The rooms given to this science, however, in the universities are usually inadequate and ill-adapted. The only such laboratory yet planned and constructed especially with regard to the requirements of this work is that at the university of Toronto, of which a description with plan is to be found in 'Science,' XIX, 1892, p. 143. The most extensive accommodation provided for this work in America is probably that at Yale, where a house with fifteen rooms is devoted to it. A description of the Yale laboratory is also to be seen in 'Science,' XIX, 1892, p. 324.

The following selected topics set recently for original investigation in two of the institutions may be taken as typical of the kind of themes through which the graduate discipline acquired in all these foundations is secured.

COLUMBIA (1893–4): "After-images—their duration and nature as a function of the time, intensity, and area of stimulation."

"The time of perception as a measure of differences in intensity, and the correlations of time, intensity, and area."

"The perception and attention of school-children."

PRINCETON (1893–4): "The progressive fading of memory for size of visual figures."

"Investigation of memory-types by means of reaction-times."

"Size and color contrast effects on the retina."

"Complex illusions of rotation." *

The treatment of general psychology is adequate as never before, also, in the graduate instruction of the country. The courses of lectures and the instruction by the Seminar method gather large numbers of students who have already graduated in less pretentious colleges. The publication in recent years of so many systematic treatises, especially in America, has

* Similar topics of research at Harvard are to be found (23 in number) in the Catalogue of the Harvard Laboratory already mentioned, and those at Yale in the "Studies from the Yale Laboratory," 1893.

contributed to this; a dominating influence in this matter being a work which has proved to be a *vade mecum* to psychological inquirers—the 'Principles of Psychology' of Professor Wm. James.

B. Pyschology as Undergraduate Discipline.—The position of psychology in the undergraduate curricula of the leading institutions also invites remark. Two important changes may be discerned in recent years, both indicating the permanent breaking away of this discipline from its earlier hampering connections: first, the recognition of the aim of the science as self-knowledge and self-control; and second, the introduction of the experimental method of instruction.

The first of these tendencies is shown in the remarkable change worked (and still working) in the qualifications and training of the occupants of chairs in Philosophy and Psychology. Even the smaller denominational institutions are following the lead of the great eastern foundations, and of the progressive state universities, in seeking men who are trained to the same rigorous interpretation of fact and search for it that are the first requisites of the genuine *Naturforscher* in other branches of science. The guardianship of this important realm, the mind, from outside, in the supposed but mistaken interests of religious and ethical truth, has had its day in many institutions—at least in any sense that denies to the investigator and teacher the full liberty of disputing hypotheses which facts do not support, and of stating those, however novel, which well-observed facts do support. Consequently Philosophy and Psychology are now self-controlling departments in the colleges; and so the courses of psychology are arranged with view both to the adequate instruction of the student in its history and results, and with view to that high discipline which the pursuit of the 'moral'—as opposed to 'physical' and 'natural'—sciences undoubtedly gives.

Second, the introduction of the experimental method of instruction has had its beginning. It consists in the actual demonstration of the leading facts of Experimental and Physiological Psychology in the class-room with added opportunities for students to perform them upon one another, and, under certain topics, upon the dissected nervous systems of

animals. One of the results is the greater concreteness and interest given to the subject for younger students and the correspondingly increased election of all the branches of the tree of philosophy in the later years. The union of the two functions of introspection and experimental observation thus secured renders this branch, in my opinion, of unique and as yet undeveloped value in the total discipline of college life.

It is evident that this undergraduate service cannot be adequately realized until the science which aims to render it is itself well developed and sufficiently categorized. The actual condition of things suggests encouragement, therefore, but not enthusiasm. It is evident that such a method of instruction is at present impossible to any but the original workers in this field, and they indeed are each a law unto himself. There are very few experiments of a psycho-physical or psychological kind which are of such evident importance and value as to be recognized by all as available for class demonstration. And a more radical defect is that there are very few principles as yet formulated which can be adequately demonstrated by single or grouped experiments. Add to this the fact that the whole exhibit of apparatus at Chicago contained very few things which are suitable and convenient for untrained use or illustration, and the difficulties become in part apparent. It is a duty which experimental psychology owes to education to meet this need by bringing her results into line with the more elementary principles of general psychology, of providing simple apparatus which can be used by less expert instructors, and of preparing text-books for junior classes. While no text-book to-day exists for this purpose, it is yet gratifying that two such 'Courses in Experimental Psychology' have already been announced by competent writers, both Americans (Professor Cattell of Columbia College and Professor Sanford of Clark University).

Reference to the latest catalogues of Brown, Wisconsin, and Michigan Universities (not to mention many others) may serve to show the nature of the courses offered in institutions where the work is as yet mainly undergraduate.

C. Psychology in its Bearings on Pedagogy.—Finally, the relation of psychology to the science of education may be given

a word after the discussion of its place in practical education. Pedagogy as a science treats of the application of psychological principles to the development of normal and cultured personality. The ground-work of such a science must be afforded therefore by psychology: and inasmuch as the teacher has to do with body as well as mind and with mind principally through the body, it is experimental or psycho-physical psychology to which this duty to theoretical education mainly comes home. It is needless to say that there is no such science of pedagogy in existence. Most of the books which have heretofore appeared in America on this topic—and their name is legion—are unworthy of serious attention. Further, the importation of the German *a priori* 'Systems of Pedagogics' finds its main service in keeping awake the expectation and the *amour penser* of teachers: not in affording them much empirical assistance in their task. Yet it is encouraging that the phrases 'child-study,' 'self-activity,' 'apperception,' 'scientific methodology,' are in the air, in this year of the Exposition, and every teachers' convention listens to hours of paper on such topics.

Contemporary psychology is becoming aware of this duty also, however far she may yet be from performing it. Children are being studied with some soberness and exactness of method. Statistical investigations of the growth of school-children, of the causes and remedies of fatigue in school periods, of the natural methods of writing, reading, and memorizing, are being carried out. The results of several such inquiries were plotted for exhibit in the department of Anthropology at Chicago. Questions of school hygiene are now for the first time intelligently discussed. The relative values of different study-disciplines are being weighed in view of the needs of pupils of varying temperaments and preferences. And it only remains for the psychologists — themselves teachers—to set the problems and establish the methods, and all the enthusiasm that is now undirected or misdirected will be turned to helpful account. Among those who have addressed themselves to this task in this country with information and influence two names may be mentioned, that of W. T. Harris, U. S. Commissioner of Education, Editor

of the 'International Education Series,' which now includes 24 volumes, and President G. Stanley Hall of Clark University, Editor of the 'Pedagogical Seminary' (vol. I–III, 1891–4). Another journal which is doing good work for sound education is the 'Educational Review,' edited by Professor N. M. Butler of Columbia College (vols. I–VII, 1891–4).

V. Psychology and Other Disciplines.

It is necessary, in conclusion, in order that this report may adequately present the conditions under which psychology exhibits herself and her historical progress, to speak briefly of the relations which this topic sustains to the other 'moral' forces which make up largely the culture element in our present-day social environment. The traditional connection with philosophy is not severed by the new directions of our effort, but on the contrary they are made more close and reasonable. The change in psychological method was due in part, as I have said above, to changes in philosophical conception; and it is only part of the same fact that scientific psychology is reacting upon philosophy in the way of healthful stimulus. Both the critical idealistic and the critical realistic methods of philosophy are richer and more profound by reason of the lessons of the new psychology. It was only just that the science which owed one of its earliest impulses in this country to a book from an advanced thinker of the former school, the 'Psychology' of Professor John Dewey of the University of Michigan, should repay the debt by its reconstruction of the Kantian doctrine of apperception in terms acceptable to the later thinkers of that school. And it is no small gain to both schools that their issue should be joined, as it is to-day, on ground which stretches beyond their old battle-fields by all the reach of territory covered by the modern doctrines of Naturalistic Evolution, and the Association Psychology. Philosophy escapes the charge of Lewes that her discussions are logomachy, when the disputants on both sides are able to look back upon those even of the late period of Lewes and admit the essential truth of both of their hotly-contested formulas. As far as this is the case, I venture to say that it is due to the

progress of psychology in giving content to the terms of the logomachy and in enabling the best men to reach more synthetic and more profound intuitions.

The relation of psychology to theology also is now as close as ever, and must remain so. And the obligation must become one of greater mutual advantage as psychology grows to adult stature and attains her social self-consciousness in the organization of knowledge. The benefits which theology might have gained from psychology have been denied in great measure through the unfortunate attempt to impose the theological method upon the treatment of the whole range of mental fact. The treatment of 'Anthropology' included in the text-books of systematic theology bears about the same relation to that of current Psychologies like Höffding's and James' as the physiology of the philosophers not long since bore to the work of the neurologists and morphologists. It is evident, however, that this condition of things is now happily mending; and it is to the credit of one man, ex-President James McCosh of Princeton College, that he first of the theologians who were teaching philosophy in this country welcomed and advocated the two new influences which I have taken occasion above to signalize as the causes of the better state of things: the influence of the German work in psychology (Preface to Ribot's 'German Psychology of To-day,' 1876) and that of the evolution theory in biology ('Religious Aspect of Evolution,' 1888).

Finally, I may note the growth of a new department of psychological study which aims to investigate the mental and moral life of man in its social and collective conditions. The evident need in such subjects as Sociology and Criminology is the knowledge of the laws of human feeling and action when man is found in crowds, orderly or disorderly, and in organizations, legitimate or criminal. This need is now beginning to be felt both by sociologists and by psychologists, and we may hope that the questions already started in Italy by Ferri, Sighele ('La foule criminelle,' 1893), in France by Tarde ('Les Lois de l'Imitation') and Guyau ('Education and Heredity,' Eng. trans. 1892), and in England by Spencer, may receive fruitful development in this country. It is an interest-

ing sign of the times in education that the theological schools are beginning to realize the need of such knowledge of collective man, as part of the training of the ministry. Instruction in social questions is made a separate department in the Yale Divinity School and in the Chicago Theological Seminary, as well as in other such institutions.

En résumé, I have only to add that psychology is now the branch of knowledge which is developing in most varied and legitimate ways; and that the exhibition made at the Columbian Exposition, while not adequate in many respects, yet served, to those who studied it intelligently, to indicate the present gains and the future prospects of the science.

DISCUSSION.

IS PSYCHOLOGY A SCIENCE?

If the differences of view between Professor James and me as to the nature, aims, and present condition of psychology were a matter of merely private import, I should not think it worth while to discuss them. But they seem to me, on the contrary, of the highest importance for the future development of the science in this country. It will not be out of place, therefore, to speak of them briefly in the REVIEW; especially since I cannot take the statement of them in his, for the most part, appreciative notice of my book as representing me fairly.

Professor James says truthfully that "the word 'descriptive' on the title-page covers more of the contents [of my book] than the word 'explanatory.'" The same thing is true of every scientific treatise on mental phenomena that was ever written; or, indeed,—for that matter, —ever will be written. Moreover, it is also true for every form of 'natural science,' from astronomy, which can describe the aurora and the sun-spots, for example, but cannot at all adequately explain them, to biology, which, with all its wealth of descriptive details, is almost absolutely powerless as yet to state any of "the logical grounds or the efficient causes of the facts described." This discrepancy between description and explanation, between our knowledge of facts and our knowledge of causes, belongs to the very nature and progress of all human science. Has my critic never read through the detailed description of the embryology of the chick; and does not he see that it is just this careful, loving regard for facts as shrewdly observed and honestly reported, in which all the modern sciences of life are perpetually laying anew the basis for their advancement?

When, however, Professor James says that there is 'very little' explanation in my book, I quite decline to acknowledge the force of his criticism; unless I am allowed to add—very little, relatively, of the *kind* of explanation which my critic thinks is the only acceptable kind. And this brings me at once to one of the chief points of variance between us. For in a somewhat emotional statement toward the close of his review Professor James declares: "I desiderate 'conditions'; for Ladd 'analysis' and 'tracing of genesis and growth' are enough." But in another place I am given rather unusual credit for skill in 'un-

ravelling the fibres of consciousness,' for realizing the intricacy of my facts with 'amazing thoroughness,' and for holding consistently to the view that 'each individual mind is an organic life that develops its own destiny.' Now has all this nothing to do with 'explaining' the phenomena of consciousness, with tracing the conditions, the grounds, the efficient causes, in the case of a study like that of the mind's life? Only the student who is predetermined to claim that cerebral psychology is the sole scientific psychology, and that the sole efficient causes are conjectural brain-states, could possibly answer 'No' to this inquiry. But such a negative answer throws out of the category of psychological science almost the entire body of the work now being done in our psychological laboratories, all the collection of statistics and all the experimentation in child-psychology, all the study of ethnic psychological peculiarities, and of the actual life of the mind, as undoubted experience brings it before us. And what does it actually substitute, or even promise, for this growing wealth of scientific material? I am quite willing that my critic's own voluminous essay in psychology, so far as it is distinctly one in cerebralistic theory, should be the answer.

Professor James distinctly declares that the 'crudest cerebralistic theories, or the wildest theosophic ones,' lead in a 'more healthy direction' than my resignation to the patient examination of the facts and to the working out carefully, in detail, of the various perplexing problems which our science proposes. This is exactly the opposite of my opinion. And if I thought that our somewhat like a score of psychological laboratories in this country were founded to serve such a method of advancing the so-called science, I would most willingly see them all perish in a single night.

How, nevertheless, Professor James can hold me up as *not* 'desiderating' the conditions of mental life, so far as they lie in cerebral and other nervous changes, I find it difficult to comprehend. Have I not written two books treating chiefly of this very subject—and one of them as big as, and even more 'tedious' than, the 'Psychology, Descriptive,' etc.? Even in this latter treatise are not the 'physiological conditions,' so far as they are known or can reasonably be conjectured, somewhat elaborately discussed in connection with almost every kind of mental phenomena? For 'the crudest cerebralistic theories,' as well as 'the wildest theosophic ones,' I admit I have no taste; nor will I ever indulge myself in such flights of mere fancy, even if I have to remain forever on the level ground of known facts, faithfully studied and made the basis of cautious generalizations. But that I should also be accused of a lack of 'craving for insight into causes,' and of desire to know about the mystery of 'the union of mind with brain,' is even yet more

strange. This, in the face of the fact that the voices of the critics have scarcely died away who accused me of taking up altogether too much space with this very problem of brain and mind,—not to speak of Professor James's own correct conjecture that I have in preparation more on the same subject.

It would appear, then, that one point of difference between us is this. I regard psychology as the science which describes and explains the facts of consciousness. Unravelling the fibres of consciousness, tracing its genesis and growth, generalizing the laws that relate its states together, expounding the conditions of every sort on which the mental life unfolds itself—this *is* for me the science of psychology. Professor James, on the contrary, holds that there is only one kind of conditions which the student of scientific psychology cares to know about, and these are the brain-states ; and that until some Galileo or Lavoisier has arisen with a psycho-physic law that will sweep the board, as it were, we can have no *science* of psychology. I, on the contrary, assert that such a view perverts and misconceives the entire subject, substitutes 'wild' and unverifiable conjecture for genuinely scientific treatment of ascertainable facts, and surrenders to the charge of being 'unscientific' by actually becoming so.

But perhaps the differences of 'temper' between us—if I may venture to use the word—are quite as strongly marked as the differences of expressed views. As to these I certainly should not remark, did I not consider that the 'cause' is somewhat interested in these differences, too. Professor James finds my whole 'sort of treatment *tedious* as few things can be tedious.' If he deemed me the only or the chief sinner in this way, I should regard his criticism as merely personal and pass it by. But he has been kind enough to say that there are many eminent psychologists who are more tedious to him than I am. And elsewhere he has spoken freely enough of 'dear old' Fechner's 'patient whimsies,' and of the intolerable 'Herbartian jargon,' to say nothing of the 'Kantian machine-shop' and the 'strenuously feeble' prattle of Professor Green. Now different things are tedious to different people —even if they are all alike students of psychology, whether from the empirical or from the speculative point of view. But is this the spirit of a genuine student of 'natural science'? Fortunately we do not all get tired in the same way. And it is not the patient unravelling of the fibres of consciousness, the careful tracing of the genesis and development of the mind's life, but rather wild cerebralistic and theosophic theories which make the genuine student of psychological science tired.

All this is, however, for only two individuals of little or no account.

It makes no difference what seems 'tedious' to Professor James or what seems interesting to me; or whether it is the same things that interest or tire us both. Such differences may be matters of temperament or of temporary digestion. But it does make a vast difference with the welfare of psychology whether the 'virgin-minded youths and maidens' are given by their teachers to understand that the patient mastery of details is indeed 'tedious'; that they themselves already carry it all in their 'bosoms'; and that it is a 'violence done to human want' to ask them patiently to acquaint themselves with facts and proved principles. When they are 'hungry for spiritual food,' their hunger should, I suppose, be fed on cerebralistic or theosophic theories; or else it should be made to wait meal-time until the psychological Lavoisier has arisen to substitute his strong meat for my 'milk for babes.'

Now he who knows American youth—their needs, their faults characteristic or readily acquired, and their fine capacities—will be sure to judge, I think, that nothing can well be worse for them than to receive gladly such instruction as this. Doubtless they do carry much psychology in their bosoms; just as they carry much anatomy and physiology in their thoracic and abdominal cavities. But at the very time when America—and young America, too—is stepping rapidly to the very front rank of the world's students of psychological science; when the youths and the maidens are offering themselves for patient and enthusiastic work at our laboratories and lecture-halls in genuine scientific spirit and method, it seems to me immensely unfortunate that one whom they so justly admire should voice such reactionary and debilitating sentiments.

In closing I will only say that Professor James's 'learned Yale co-editor' is ready to join him in 'ringing in' the expected 'Lavoisier,' with his grand generalization that shall lift our knowledge of mental behavior to a level with that certainty which the behavior of the planets has for the mind that knows the law of gravitation; *if only* this great reformer of psychology ever arrive. But in this region he is not expected. And meantime we shall not spend our time longing and sighing for him. Meantime we shall go plodding on as best we may, even if we are thought tenfold more 'tedious' by our brilliant colleagues, in the future than in the past. Besides all this, we believe that the *science* itself, which is more than any Lavoisier, is with us now; and that this same patient study of facts, and this earnest but slow pushing forward of the realm of law on the basis of such study, is the very thing in behalf of which the bells should be rung, if they are to be rung at all.

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THE BEARING OF THE AFTER-IMAGE.

A few years ago Dr. Carl Hess examined carefully the change of tone produced in a given spectral color by the circumstance that the eye had already been fatigued by gazing at another color. The experiment was conducted in the following manner. The observer looked into the telescope of a suitably arranged spectroscope, and saw in one half of the field a given color, the fatiguing color; by fixating a minute bright point in the centre of the field, the effect of this color was confined to one half of the retina. After a certain lapse of time (varying, in the different experiments, from 30 to 75 seconds) another color, the reacting color, was allowed to fall upon this same half of the retina, and upon the other, unfatigued, half such a spectral color as the observer found, by moving the collimator-tube, to match it exactly. The wave-lengths of the three color-tones, the fatigue-color, the reacting-color, and the comparison-color, were all taken; hence the experiment would seem to be of a commendable degree of accuracy, and not to consist only of *schwankende Schätzungen*, to use Helmholtz' phrase in criticising it.

A large number of trials were made, of which the following may serve as examples:

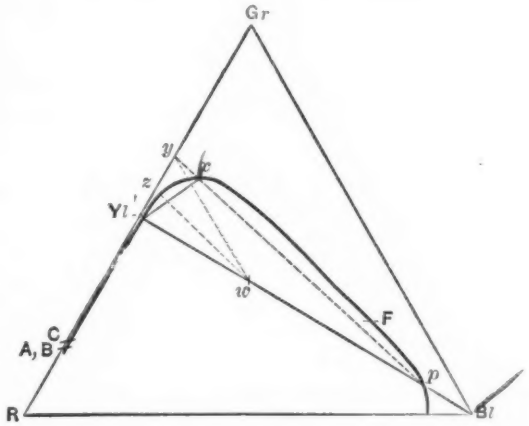
After looking at red, violet becomes bluish green, green becomes greenish blue.

After looking at violet, red becomes reddish yellow.

After looking at blue, red becomes reddish yellow (more yellow than above).

It will be seen that in each of these cases (as happened in all the cases tried) the effect is the same as if the reacting color were mixed with a very considerable amount of the color complementary to that first gazed at. Now this *fact* is nothing new for Helmholtz; he mentions it explicitly in the first edition of the *Physiological Optics* (p. 368). The question is whether the explanation given by Helmholtz of the after-image which causes the result is sufficient to account for the amount of change produced. The situation can be made plain with the aid of a diagram representing König's color-triangle. Suppose that fatigue has taken place for the fundamental blue, and that a slightly yellowish green, x, is then looked at. In accordance with the principles of the color-triangle, the effect upon x of a diminution of the blue contained in it is found by drawing a line from Bl to x, and producing it beyond x. But if this line be produced to the very

border of the color-triangle,—that is, to the very limit of sensations ever experienced; in other words, if the power of the retina to produce the blue sensation is completely exhausted, the sensation which results cannot be beyond that represented by *y*, whose color-tone, found by joining *y* to *w*, differs very little from that of *x*. But the actual color-tone observed is found to be somewhere near the line *zw*,



and quite saturated; hence it can only be accounted for on the hypothesis that there has been a very considerable admixture of a saturated yellow. For Helmholtz, this can only be obtained as a residue of the self-light of the retina after it has been exhausted for blue. For the after-effect of blue upon red the case is still worse, since spectral red contains absolutely no blue. Helmholtz himself distinctly says, in the new edition of the *Physiological Optics*, where he discusses this work of Hess (p. 518), that for a theory in which any of the spectral colors corresponded to only one, or only two, fundamental colors, or for which the self-light of the retina was assumed to be small, the objection would be justified. In his present paper* Hess shows that, even with these conditions, the cause assigned is not sufficient to account for the effect; that even if the exhaustion had proceeded so far that the blue first looked at had turned to gray (what was far from being the case), and if the amount of the self-light was much greater

* *Ueber die Unvereinbarkeit gewisser Ermüdungserscheinungen des Sehorgans mit der Dreifasertheorie.* C. Hess. Archiv für Ophthalmologie, XXXIX. 2, 45–70. 1893.

than is usually attributed to it, it would still be impossible to account for the presence of a yellow of so much color-quenching power.

As already said, the phenomenon here exhibited has already been stated by Helmholtz to occur, and to its full extent. There is a simple experiment of Hering's, also, which ought to become classical, in which the amount of the after-effect is shown in a way which cannot be improved upon: one has only to look fixedly upon a patch of color illuminated by the light of a lamp, and then to partly *turn down the lamp*, when the patch, still looked at with open eyes, will appear to be spread over with the complementary color. This shows beyond question that the after-effect is something of very considerable intensity. While this paper of Hess, therefore, makes no distinct addition to our knowledge of facts, it is still very important in calling renewed attention to the necessity for admitting that when an after-image is produced, some very pronounced physiological process is taking place in the retina. But this does not establish an incompatibility with the Young-Helmholtz theory. Helmholtz at present assumes a photo-chemical process in the retina, the nature of which he does not farther particularize. There is no reason to suppose that, if he found it desirable, he might not add some such assumption as would enable him to assign a physiological basis to the after-image. It is not necessarily a fundamental part of his theory to give so much play as he does to illusions of judgment. They must evidently play some rôle, for everywhere else the principle of the relativity of sensation is extremely important; if Helmholtz proves to have been mistaken in assigning to them too important a rôle as an intensifier of after-images, it is, beyond question, a mistake that might easily be corrected. It is an error of reasoning far too commonly made to suppose that all explanations of phenomena given by the individuals who uphold a theory are an essential part of the theory, and that to discredit them is to upset the theory itself. And in particular there is nothing in the facts adduced by Hess which is incompatible with a 'three-fibre' theory, but only with the explanation which Helmholtz has hitherto given of the after-image.

We have pointed out in the May number of this REVIEW (page 324) that the case is the same for the phenomena of simultaneous contrast; that they are of a physiological nature is not incompatible with the Young-Helmholtz theory (and still less with every three-color theory), but only with the attribution of an unnecessarily exaggerated importance to an admitted effect of illusion of judgment. It is essential to set this forth with explicitness in the interests of sound reasoning, but it must not be taken as giving any appreciable amount of aid and comfort to the upholders of the Young-Helmholtz theory. The single

fact that that theory requires us to interpret white (not the physical cause of the sensation, but the sensation itself) as a mixture of red, blue, and green is enough to make it an absolutely impossible theory to the psychologist. So the fact that Hering's theory requires us to attribute equally useful functions to decay and to growth makes that an impossible theory to the physiologist, except as the last resource of an exhausted scientific imagination. But every one who is interested in these theories is either a physiologist or a psychologist. Hence the necessity of substituting for them both an hypothesis free, upon the threshold, from objection of so critical a nature.

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PSYCHOLOGICAL LITERATURE.

SOCIAL PSYCHOLOGY.

The Psychic Factors of Civilization. LESTER F. WARD. Boston, Ginn & Co., 1893. Pp. xxi + 369.

Social Evolution. BENJAMIN KIDD. New York and London, Macmillan & Co., 1894. Pp. vi + 348.

Civilization during the Middle Ages. GEORGE B. ADAMS. New York, Charles Scribner's Sons, 1894. Pp. vi + 463.

History of the Philosophy of History. ROBERT FLINT. New York, Charles Scribner's Sons, 1894. Pp. xxvii + 706.

An attempt to state the foundations of a sociology definitely based upon psychological methods and data has an interest for psychologists quite independent of its worth for students in the social field. This interest is a double one: it is worth while to see what sort of psychological ideas are used to lay the basis of another science, and it is worth while to note the reaction of their social application upon the ideas themselves—to note, that is, how psychological ideas look when handled by one whose chief interest is in their efficiency to explain the development of social life. Accordingly I shall consider Mr. Ward's work on both sides: how in his essay psychology contributes to sociology, and how sociology in his hands supplies valuable data to the psychologist. And if I am led to the conclusion that Mr. Ward gives back considerably more to the psychologist than he succeeds in borrowing from him, the conclusion only adds to the psychologist's interest in the work, however it may square with Mr. Ward's intention.

There are two questions of paramount interest in sociology: one, the question of the nature of the social forces; the other, the question of their control. As it happens, both of these questions are psychical questions. The force which keeps society moving is a psychical one, the 'soul,' using the term *soul* not in a theological or even technical philosophical sense, but in its popular meaning—the feelings taken

collectively. The power which gives direction to these forces is also psychical—the intellect. Now, on one hand, according to Mr. Ward, these considerations suffice to overthrow the reigning *biological* method in sociology, as represented by Spencer, in its theory, and still more as to the practical conclusions (*laissez-faire*) drawn from it. As to this—save to suggest that possibly Mr. Ward takes Spencer somewhat more seriously than a psychologist would take him, and to regret that the somewhat irritating self-consciousness of Spencer's style should occasionally have infected Mr. Ward's way of putting things—we have nothing to do. We are concerned with his subjective psychology, or account of feeling as psychical motor, and his objective psychology, or intellect as psychical director.

Mr. Ward's psychology of feeling and action is a compound (not a happy one, as I shall try to show) of the old-fashioned psychology of sensation, dating from Locke, and Schopenhauer's theory of will. The crudeness of his account of sensation and idea may best be gathered from his own words: "When the end of the finger is placed against any material object two results follow. There is produced a *sensation* depending upon the nature of the object, and there is conveyed to the mind a *notion* of the nature of the object" (p. 16). If the sensation is indifferent as to pleasure and pain, attention will be fixed upon the notion conveyed, and abstracted from the sensation. In this case perception occurs. What sort of thing the percept of an object will be independent of the qualitative character of the sensation, Mr. Ward does not try to say: he only tells us that "the sensation and the notion are not one and the same, but two distinct things." This complete dualism (he tells us of the 'dual' nature of mind, p. 12) lies at the basis of his conception of feeling as psychical and social force, and intellect as directing power.

While indifferent sensations are neglected for the notion which they convey, the intensive sensations meet a different fate. Pleasure and pain are connected with them, and this fact occasions movement: movement which is definite and purposeful *—away from object when there is pain, towards it when pleasure. These acts are the simple impulsive movements. But besides this sensori-motor apparatus, there is an ideo-motor apparatus, which gives rise to rational acts. These acts, Mr. Ward asserts (p. 33), come as clearly as the sensori-motor within the generic definition of being the result of sensations and away from pain and towards pleasure. This may be true; but how it can be true with-

* If this be true in the unqualified way in which Mr. Ward states it, it is difficult to see why the intellect should ever be needed to 'direct.'

out a complete reconstruction of his original dualism between the 'subjective' sensation and the 'objective' idea, I fail to see, the 'idea' having been defined as wholly without pleasurable or painful quality.

Desire is the next stage of development, and is 'the recorded and remembered pain and pleasure.' Since the representative states are much more important in our life than presentative sensations, our whole being becomes a theatre of desires seeking satisfaction, but checked in many ways, so that there results a perpetual striving to obtain the objects of satisfaction. From this time on the psychology of will completely supersedes that of sensation: the appetites of hunger and thirst, love, æsthetic and moral cravings, all springs to action, are included under desire, and language is strained to exhibit "all animated nature burning and seething with intensified desires" (pp. 52–3). We are next told that all desire is a form of pain, while effort aroused by desire is simply to satisfy it, that is, allay the pain. "All the enormous exertions of life are made for the sole purpose of getting rid of the swarm of desires that goad and pursue every living being from birth to death" (p. 55). That a remembered pleasure as well as a remembered pain should be of itself desire, that it should be pain (not simply painful), and that of itself it should know how to terminate itself, and that this termination should be pleasure—all this will probably strike the psychologist as curious enough; but the end is not yet. The satisfaction of desire terminates it, and the subject returns, psychologically, to its previous condition. But this of itself leads to the pessimistic conclusion: the sole spring to action is desire, desire is pain, and the satisfaction of desire is simply the cessation of pain. Yes, replies Mr. Ward, all this would be true if the act of gratifying a desire were absolutely instantaneous (p. 65). But the sensation of gratification is continuous; it takes time; in the higher form of love, indefinite time. "So long as the object is present the pleasure abides" (p. 68). Now I do not intend to question this as a fact; but, again, I do not see how the statement can be true if Mr. Ward's previous psychology be true. All gratification of desire implies the presence of desire; a non-existent desire can hardly be gratified, and all desire is pain. *Ergo*, as long as there is gratification there is pain—at most a mixed state of pleasure-pain. This is Mr. Ward's only logical conclusion. The *object* whose permanence gives permanent satisfaction is a visitor from another sphere than that of sheer feeling which forms, with memory, the whole of Mr. Ward's data. The contradiction becomes oppressive when we are further told (p. 74) "that, provided the means of supplying wants can be secured, the greater the number and the higher the rank of such wants, the higher the state of happiness attainable."

While feeling (pleasure) is the result of desire *psychologically* (or for the individual), *function* is the result so far as nature is concerned. The satisfaction of the desire to eat builds up the whole system's further structure, and that develops organic function. There is still another result, *totally* (p. 79) different from either feeling or function. In satisfying desire the individual puts forth *action*, and this is a condition of building up structure. It is the connecting link between pleasure and function—the consequence of the former, the condition of the latter. The transformations thus wrought constitute material utilities, material civilization. Of these neither the individual nor nature is the beneficiary, but society. Thus there are three distinct ends—function for nature, pleasure for the individual, and action, with its products, for society.

I mention these points for their negative rather than their positive value. All these separations, with the contradictions previously indicated, result logically from the original premises. Let the fundamental thing be conceived as impression resulting from contact with an object, and thought, perception, must be another sort of thing; desire and action can be brought in from passive feeling only by a virtual contradiction, while nature, the individual, and society have independent ends.

For, to begin with the last point, it is simply the insertion of a passive impression between the 'object' and the feeling and idea that makes such a break in the respective ends of nature, individual, and society as Mr. Ward introduces. Let once the standpoint of *action* be taken and there is a continuous process: the sensory ending is a place, not for receiving sensations and starting notions on their road to the mind, but a place (viewed from the standpoint of nature) for transforming the character of motion; the brain represents simply a further development and modification of action, and the final motor discharge (the act proper) the completion of this transformation of action. Whether the discharge is sensori-motor or ideo-motor depends simply upon the intermediate transformation which the original motion undergoes. Now while the psychological description of the process may employ different terms, it cannot involve a different principle. To suppose that feeling starts off action attributes a causal power to a bare state of consciousness at which many of the 'metaphysicians,' before whom Mr. Ward so shudders, would long hesitate. What feeling adds is consciousness of value of action in terms of the individual acting. While this appreciation of value marks a tremendous factor in the development of life, it is altogether too much to suppose that its introduction means the introduction of a new agency: the

abdication of 'natural' energy (motion) and the substitution for it of a new power-feeling.*

Furthermore, there is no reason to make function the 'end' of nature: its 'end' (like its beginning) is activity, or motion; the structural organization (and the corresponding functioning which goes along with it) being simply the objective manifestation of the transformation of motion. Even from the standpoint of 'nature,' function (or rather structure, which I take Mr. Ward to mean, since function always *is* action) is instrumental, not final. Only because Mr. Ward tries to get action out of passive states of feeling (pleasures and pains) does he have to reverse this natural order, and make action the intermediate term between feeling (the individual's end) and function (nature's end). Once adopt the united and continuous standpoint of action, and our three different ends resolve themselves into one—an end which may be termed valuable (felt) functional activity.

It probably is hardly necessary to deal at length with the weakness of Mr. Ward's treatment of original and representative action. The ignoring of impulse, save as representative, or the memory of previous pain and pleasure; the reduction of both ideo-motor and sensori-motor action to response to feelings of pain and pleasure, leaving out of account both the qualitative side of sensation and ideas, and also the connection of sensation (directly) and ideas (indirectly) with impulse; the account of desire as representative pleasures, which are suddenly asserted to be a state of pain; the abrupt appearance of permanent objects of satisfaction—all this is its own sufficient commentary.

When, however, we remember that Mr. Ward's original text is the need of relatively less attention to the intellect and more to the motive side of mind, and that his object is to get a basis for social dynamics on the side of its motor powers, we have an instructive object-lesson. All this unsatisfactory and self-contradictory analysis results from the fact that Mr. Ward is so under the spell of an old psychology of sensation that he fails to recognize the radical psychical fact, although just the fact needed to give firm support to his main contentions—I mean *impulse*, the primary fact, back of which, psychically, we cannot go.

* It may avoid misapprehension here if I remark that I am not arguing that the 'external' motion is the cause of the 'internal' state of consciousness. To treat one as *cause* of the other is to suppose one independent of the other, and thus to break the continuity. My point is, that if one chooses to take the standpoint of physical science and describe as far as possible the psychical occurrence, this occurrence is one of the transformation and complication of motion. The fact of feeling and of the existence of ideas must be recognized, but they must be treated from the standpoint of the development of action.

XUM

Starting with impulsive action, Mr. Ward would have, I think, no difficulty in showing the secondary or mediate position occupied by intellect. In order to secure this, his main purpose, he could well afford to sacrifice both the theory of feelings of pleasure-pain as stimulus to all action, and the old myth of sensation somehow walking from the object over into the mind. He would secure both a consistent psychology and a unification of the ends now attributed to three different existences by a psychology which states the mental life in active terms, those of impulse and its development, instead of in passive terms, mere feelings of pleasure and pain.

It is a pleasure to turn from these somewhat negative results to the other field—the light which Mr. Ward throws upon psychology from the standpoint of sociological evolution. I must omit more than bare reference to Mr. Ward's account of the reaction upon environment resulting from the introduction of specialized psychical phenomena. The points he makes (pp. 84–89) regarding the effects upon vegetable life in the way of the evolution of flowers and fruit, of the appearance of mind (in insect and bird organisms), and concerning the effect upon physical characters, including the brain, of the male animal of the development of sexual appetite in the female, are well worth attention.

But Mr. Ward's main contribution in this direction is in the theory which he propounds regarding the growth of intelligence, and the differentiation of the male and female types. It would perhaps hardly be safe to say that there is anything absolutely original in the points urged by Mr. Ward, but I do not know any writer who has made them in so striking and effective a way.

The key-word to the whole evolution of mind is *advantage*. Gain consists in increased ability to satisfy desire; hence the arousing of direct effort, of that striving which we call brute force. But many desires cannot possibly be satisfied by the primary method of direct effort. When a desire having a certain amount of active vigor at command meets obstacles, the result is that the animal is no longer simply checked; while external motion is arrested internal motility is increased. In this way the animal may continually change its position or point of attack, and thus by an indirect or flank movement finally reach its goal. This advantageous method would be selected and perpetuated until, finally, the power of mental exploration is developed. This incipient power leads up to 'intuition,' defined as the "power of looking into a complicated set of circumstances, and perceiving that movements which are not in obedience to the primary psychic force are those which promise success."

Intelligence is thus indirection—checking the natural, direct action,

and taking a circuitous course. This accounts for the touch of moral obliquity attaching to all words naming primitive intellectual traits—shrewdness, cunning, crafty, designing, etc. It also accounts for the large part played by deception in historic social life—military strategy, political diplomacy, and, at present, business shrewdness. It is the legitimate consequence of this stage of mental development. So far as nascent intelligence is directed towards other sentient organisms (as it is where the getting of food or avoiding of enemies is concerned), intelligence is egoistic, living at the expense of other organisms. But a further development takes place when it is directed to inanimate objects. Ingenuity is substituted for cunning, and in so far intelligence becomes objective, impersonal, disinterested. When the savage makes a bow and arrow, his ultimate aim, indeed, is still gratification of appetite; but for the time being his attention must be taken up with a purely objective adjustment—with perception of relations of general utility, not of simple personal profit. In this way intelligence gradually, through the mediation of invention, works free from subjection to the demands of personal desire. It sets up its own interest, its own desire, which is comprehension of relations as they are. Scientific discovery and speculative genius are simply farther steps on this same road.

The ordinary biological theory of society does not see beyond the egoistic, exclusive development of intelligence. Its practical conclusions are, therefore, all in the direction of *laissez-faire*. But a psychological theory must recognize the change in the conditions of evolution wrought by the development of the non-personal, objective power of intelligence. True legislation is simply the application in the sphere of social forces of the principle of invention—of objective co-ordination with a view to increase of efficiency, and preventing needless waste and friction. Given a social science and a psychology as far advanced as present physical science, and *laissez-faire* in society becomes as absurd as would be the refusal to use knowledge of mechanical energy in the direction of steam and electricity. Mr. Ward, however, does not hold that psychology justifies the extreme socialistic conclusion, but rather leaves action a matter of specific conclusion: Let society do as the individual does—do what seems best after detailed study of the relevant facts. This seems good sense, but I doubt if Mr. Ward has duly considered the possibility of this outcome if, as he has previously urged, society has one end, viz., action, and the individual has another, feeling. If this opposition of ends exists, any possible development of intelligence can, it seems to me, only bring the conflict into clearer relief, and bring out definitely the necessity of choosing whichever is

considered more important and sacrificing the other. In other words, what is needed is not the substitution of a psychological theory (in terms of individual feeling) for the biological theory (in terms of function), but rather an interpretation of the latter into its psychological equivalents—a theory of consciously organic activity.*

At an early period a differentiation into two main types of intuition occurs: male, whose course we have already followed, and female. Male intuition develops with reference to reaching remote ends; it works out means; it is essentially planning or contriving. It develops new schemes, etc. Female intuition develops with reference to the immediate present; it is a question not of getting food at a distance, against obstacles, but of protecting herself and young against present danger. Female intuition develops, therefore, in the line of ability to 'size up' the existing situation; it reads signs: it is essentially interpreting, not projective or contriving. This seems to me the nearest approach yet made to putting the psychology of the sexes on something approaching a scientific basis. When Mr. Ward goes on to argue that the male intelligence is radical, the female conservative, I cannot follow him so unreservedly. It seems to me that both the facts and a legitimate deduction from his own theory justify the conclusion that the male intelligence is radical as to ends, but cautious as to immediate methods to be followed—that is, while entertaining new projects easily, is slow in coming to a conclusion as regards their execution. The peculiar abstractness of the male intelligence results from this combination. The female intelligence, while hesitating in the consideration of radically new ends, is decidedly radical in its adoption of means with reference to ends—its tendency is to take the shortest course, irrespective of precedent. The prevalent theory of the essentially conservative nature of woman's intelligence seems to me a fiction of the male intelligence, maintained in order to keep this inconvenient radicalism of woman in check.

I cannot conclude without adding that Mr. Ward's book is extremely suggestive—as well for what it does not accomplish as for

* Before passing on to the next topic, I wish to remark that Mr. Ward's general theory of the evolution of intelligence seems to me to promise a much more hopeful reconciliation of the *a posteriori* and the *a priori* than Spencer's method. The 'raining in' of an external environment upon the organism until its main features are reproduced in the organization of the latter offers more difficulties than it solves. From Mr. Ward's standpoint, the development is always controlled by the organism itself—it occurs in the process by which the latter reaches its own end, and in that sense (probably the only tolerable one) is *a priori*, while the whole process is itself an experimental one.

what it does. Its moral (to my mind) is pointing to a step which the book does not itself take. The current theory of mind undoubtedly needs reconstruction from the sociological standpoint; it needs to be interpreted as a fact developing with reference to its social utilities. The biological theory of society needs reconstruction from the standpoint of the recognition of the significance of intellect, emotion, and impulse. Mr. Ward seems to me, when all is said and done, to give a compromise and mixture of the two older standpoints, rather than a re-reading of either of them.

Three ideas run through and through Mr. Kidd's book, repeated and intertwined without much regard to the logic of formal presentation, and yet so put each time as not to convey the effect of wearisome reiteration. These ideas are: I. Progress is always effected through competition and struggle. There is infinite narrow variation, some variations tending slightly below, others slightly above, the existing average standard. There is in these variations no essential tendency to progress. Progress comes only through selection of favorable differentiations, and there is no selection save where there is rivalry and struggle. This biological law (with regard to which Dr. Kidd follows Weismannism in its extreme form) holds of human as of animal history. Its scene of operation is simply transferred to the rivalry of nations and of industrial life.

On this point Mr. Ward and Mr. Kidd seem to me to provide necessary correctives of each other. The positive evolutionary significance of conflict seems hardly to be recognized by Mr. Ward; he seems to think that intellectual progress can now cut loose from the conditions under which it originated, namely, preferential advantage in the struggle for existence. To me it appears as sure a psychological as biological principle that men go on thinking only because of practical friction or strain somewhere, that thinking is essentially the solution of tension. But Mr. Ward is strong where Mr. Kidd appears defective: in the recognition of the part which coherent, organized science can play in minimizing the struggle, and in rendering effective that residuum necessary to maintain progress. The elimination of conflict is, I believe, a hopeless and self-contradictory ideal. Not so the directing of the struggle to reduce waste and to secure its maximum contribution. It is not the sheer amount of conflict, but the conditions under which it occurs that determine its value. Mr. Kidd seems practically to ignore this possibility of increasing control of conflict, and to leave the individual at its mercy; the individual, according to him, is a tool of the conflict in evolving progress, not the conflict a tool of man.

This brings us to the second point. II. Progress implies the sacri-

fice of the individual to the race; the individual has to suffer from the conflict in order that the race may enjoy the benefits of progress. This position of itself offers nothing new; the problem has been felt ever since man became conscious of progress. The contention between Herder and Kant in Germany, between Malthus and the 'perfectionists' in England, represent it. But the use to which Mr. Kidd puts the idea is, so far as I know, original, and marks a mind of scope and daring. As man becomes conscious of the extent to which he is sacrificed to a progress in whose benefits he does not share, and as he gains in rational power, he will squarely propound to himself this problem: Why should I continue to suffer simply for the sake of progress? Go to; let us make the best of the present and eliminate struggle and conflict. And from the standpoint of reason this position is logically justified; there is no *rational* sanction for progress. This is the psychological basis of socialism, for socialism is simply extreme rationalism applied to the existing conditions of life. It proposes to put a stop to the suffering which struggle inflicts on individuals; though this implies a brake on progress.

III. Where then is the sanction for progress, science, or rational method utterly failing to justify it? In feeling subjectively, or religion objectively. The sociological function of religion is to cultivate in the individual passive resignation to or even active co-operation in his sacrifice to the good of future generations. Only in this way can the universality, historical and psychological, of the religious consciousness be explained. The scientific man in his ignoring of, or attack upon, religion fails to notice this sociological, evolutionary meaning, and indirectly plays into the hands of the socialist.

I have given, I think, a fair account of Mr. Kidd's main intentions; what I have not given is his force of statement and his wealth of illustrative material. Any detailed criticism upon such radical and far-reaching propositions is out of the question, but I cannot refrain from two suggestions. If the individual is *continually* sacrificed to the conditions of progress, where is the progress? Mr. Kidd speaks as if sacrifice to progress and sacrifice to welfare of future humanity were the same (see p. 291). But this cannot be; the benefit which will accrue to the future generations must, when their turn comes, be incidental to the sufferings attendant upon conflict as a condition of further progress. The process never amounts to anything, never has any value, *unless it has it both now and then*, i.e., all the time. Mr. Kidd seems to me to have fallen into the old pit of a continual progress *towards* something. This indicates my second suggestion. The antithesis which Mr. Kidd makes between what constitutes the happiness of the individual and the

conditions of progress appears to be overdrawn and out of perspective. Overlooking the fact that the sense of contributing to progress is an important, and to many an indispensable, rational ingredient of happiness, what ground is there for the assumption that the individual's rational conception of happiness excludes all suffering arising from struggle? I do not see that the case stands otherwise for the conditions of happiness (individual welfare) than for the conditions of progress (general welfare). A certain intensity and, so to speak, tautness of activity appear requisite to happiness; and rivalry or struggle, for anything we know, is as constantly necessary to keep us strung up to the proper pitch for happiness as it is to afford the conditions which enable preferential selection (progress) to act.

All this is upon the supposition that Mr. Kidd is justified in his extreme Weismannism of premise. If we suppose that consciously acquired activity, and habits formed under the direction of intelligence, are conserved, the case against his point is much strengthened. While struggle and consequent pain are not eliminated, the vibration is so loaded by established habits as to lessen its range. There is even no need to suppose that the conservation of rationalized activity is direct or through the organism; if the environment is so changed as to set up conditions which stimulate and facilitate the formation of like habits on the part of each individual, the same end is reached.

I hope it will not seem an injustice to Professor Adams's lucid and substantial piece of work if, after having called attention to its helpfulness to students of intellectual as well as of political development, I use it to point a moral for psychologists. As giving an adequate and coherent account of the general conditions and movement through the middle ages, the book is highly valuable to any one who is trying to understand the philosophy of that period. But from a narrower psychological standpoint the value is, in the main, negative. I mean that it indicates the slight extent to which psychology has as yet penetrated into the sciences which lie nearest to it, the historical. Psychology has not as yet made of itself a generally useful tool; it has not impressed the worker in other fields so that he feels the necessity of keeping his eyes open for the psychical development, the growth in consciousness; nor does it give him much help when he does attempt this. To take one point: Professor Adams recognizes clearly the great significance of the middle ages in discovering the individual and bringing him to the light of day (pp. 91, 92). But this is treated mainly as an objective change—a change in political status. The extent to which this depended upon a changed psychical attitude, and the part played by the implicit and explicit psychological theory of mediæval thought

—all this does not meet recognition. And yet this seems the key to understanding the outer transformation. Now this, of course, is no reflection upon the historian; he cannot be expected to stop historical investigation in order to make for himself adequate psychological instruments; it is, once more, a warning and a stimulus to the psychologist.

It is hardly necessary to do more than to call attention to Professor Flint's noble beginning of a monumental work. The present volume (of 700 pp.), after an Introduction dealing with Greek and Roman speculation upon history, is devoted to the philosophy of history in France, and we are led to anticipate further volumes upon England, Italy, and Germany. I cannot pretend to have the knowledge required to speak critically of this book; indeed, so wide is its range and so thorough its treatment that I do not see that anybody but Professor Flint himself is competent to speak of it as a whole. It seems, however, to have all, and more, of the solidity, accuracy, and restraint of judgment which marked the older volume upon Germany and France. To risk one more *obiter dictum*, judging from the accounts of authors with whom I have some acquaintance, as Rousseau and Comte, the book is more likely to be serviceable as a statement of the facts of the case than as an account of the underlying *motif* and trend—as interpretation, in short. But this simply means, I suppose, that I should interpret it otherwise myself. J. D.

Pain, Pleasure, and Æsthetics. An Essay concerning the Psychology of Pain and Pleasure with Special Reference to Æsthetics. By Henry Rutgers Marshall. Macmillan & Co., London and New York, 1894. Pp. xxi + 364.

This book, parts of which are already known to readers of *Mind*, contains at least two notable contributions to psychology. One is a new formulation of the differentia of the æsthetic judgment, a new definition of beauty. The other is a physiological hypothesis about the basis of pleasure and pain, together with the deduction from it, by way of verification, of various æsthetic laws. The reader stands in some need of emphasizing these chief points and of sustaining his interest by keeping them in mind, because the book is not compactly arranged and there are some arid stretches to traverse. One cannot help regretting that there is so little pruning and illustration, and so many phrases like 'the indifferent state of sleep-unconsciousness,'* 'joy-pleasure,'† 'non-expectation painfulness,'‡ and 'pleasure-pain toned.'§ But it must be confessed that in this scorn of the graces

* p. 327. † *Id.* ‡ p. 235. § p. 237.

Mr. Marshall follows good precedents. Philosophers writing about beauty have seldom made their books an illustration of their subject.

The attempt to define beauty objectively, we are told, has failed. No characteristic can be found common to all beautiful things except the power of pleasing us. In this relation to our pleasure, then, we are to see the source of æsthetic values. Æsthetics thus appears as a subdivision of hedonics, and the question is now only to find the differentia which distinguishes æsthetic pleasures from those of other kinds. And here Mr. Marshall makes a distinction which he advances with his accustomed modesty, but which we are inclined to regard as one of the most valuable points in the book. It marks a rare freedom from the convention and routine of abstract thinking and a fresh perception of psychological fact. The distinction is one between the æsthetic impression—the beauty which we see and feel in the moment of perception—and the æsthetic judgment—the beauty which we attribute to the object upon subsequent reflection. Into the first enter all the pleasures we may be having at the time; there is none which does not merge with our enthusiastic recognition of a present beauty and which does not add warmth and sincerity to our delight in the object before us. But the parts of this psychosis are not equally re-presentable. Some will never be recalled, and the pleasure in particular which forms a part of our consciousness may be detached entirely from the object when this object recurs in the memory. The memory of things pleasant is often painful, and the memory of things painful pleasant. When, therefore, we are called upon to pronounce a deliberate judgment on the æsthetic value of a remembered object, we shall no longer find the same pleasures in our mind to form the source and substance of our approval; some of these pleasures will surely have disappeared, and only some, if any, will remain. Of these pleasures which recur with the revival of the object our permanent judgment of beauty must be made up. Those pleasures are therefore æsthetic which are relatively permanent in revival.

Striking as the distinction is between æsthetic experience and æsthetic opinion, the definition of beauty founded upon it seems open to a double objection. In the first place, granting that all concomitant pleasures add to the beauty of present objects, there remains a clear distinction between the sense of pleasure and the sense of beauty. If, for example, I have no definite object before the mind, but am lost in a torpid reverie, I might still have pleasure; but this pleasure would not be æsthetic, because I could not perceive any beauty, seeing that no object is present to me in which that beauty may reside. Our author seems to make the distinction between pleasure and beauty impossible

for direct perception, where it is nevertheless clearly found. He thinks that a pleasure is always æsthetic if it is attached to an image in the memory. But here again the facts stand in the way. For few pleasures are so vivid in revival as those of satisfied vanity, affection, revenge, and other personal passions; it is easier to summon them up than to recall the feelings produced by a work of art. In fact, many of our strongest passions are ideal, and feed and grow upon imagination, while taste can establish itself only by constant renewals of observation, shifting tests of sense, and attentive return to the reality of experience.

If pleasure is the source of beauty and æsthetics a department of hedonics, the nature of pleasure is evidently a matter of fundamental importance for the æsthetician. Mr. Marshall accordingly devotes the greater part of his book to the discussion of the nature and physical basis of pain and pleasure. He examines and rejects the classification of pleasure and pain as sensations and as emotions, and defines them himself as qualities either of which may, and one of which must, belong to every perception of the mind. That is to say, there is nothing in the nature or character of any idea which makes it essentially agreeable or disagreeable: the object may remain the same while its value changes. But some value, negative or positive, it must always have, and must affect the mind with some tinge of pain or pleasure. This part of the discussion, which occupies the first chapter, turns, it seems to me, upon a matter of words. Introspection cannot distinguish perceptions from the qualities of perceptions. The same fact, for instance, is described equally well by saying that color is a quality of extension, or that they are two simultaneous perceptions, or that they are both qualities of a present substance. The question becomes real and not verbal only in the field of physiology. If one organ gives the idea of extension alone and another organ that of color alone, then the perceptions are separable; we have but to inhibit the action of one organ to prove it. And so it seems to be also with pleasure and pain. If there are specific nerves that yield these feelings and nothing else, they are independent sensations; if every nerve, as Mr. Marshall believes, yields a further sensation which may be tinged now with pleasure and now with pain, according to the circumstances of its excitement, then pleasure and pain are qualities of sensation, incapable of appearing by themselves. Apart from physiological evidence, which is not here presented, the discussion of such a point seems to be futile.

We pass at once, then, to the author's physiological hypothesis. It is that when an organ responds to a stimulus by the discharge of surplus stored force the activity gives pleasure; when the stored force is

inadequate to meet the demands of the stimulus we have pain. The significance of this formula evidently depends upon the fixing of the idea of the *normal* reaction; we must know how much force is necessary, and just where the storage of surplus force begins. To say that the stored force was inadequate when we feel pain, and that it was superabundant when we feel pleasure, would be to fall into a vicious circle. Mr. Marshall is perfectly aware of this danger, and seeks to avoid it by an appeal to habit. The processes of nutrition go on at a given rate; the stimulus may cause a discharge that will practically balance the nutrition, and in this case we have practical indifference in consciousness. If the stimulus falls short of this strength, the discharge will not counterbalance the nutrition, and there will be a storage of energy in the organ; this is the surplus force. If the stimulation is now increased to more than what it was in the first and normal case, we shall have pleasure, because the organ will be ready to meet this unusual stimulus with its stored energy. Had this stronger stimulus, however, been applied before the storage of the surplus energy, the result would have been pain; for the organ would have had no surplus on which to draw, and would have found the stimulus excessive and ultimately destructive. An unusual demand need not always produce destructive action, because it may attract increased nutriment to the organ affected; and by this increase of the rate of nutrition the standard of normal action will be raised, and the given stimulus will cease to be excessive. In this way we understand how pain by its very nature tends to disappear.

Mr. Marshall's formula thus proves to have a good deal of elasticity and suggestiveness. If it does not wholly emerge, even at the end, from its inevitable vagueness, it is at least developed with great ingenuity, and many facts, at first sight rebellious to it, are explained as its corollaries. The pleasures of relief, for instance, are attributed to the return of activity in organs other than that which caused the pain, organs in which action had been inhibited and surplus energy consequently stored. Introspection surely accords with this explanation, and our author adds one to the strong attacks upon the theory of contrast and total relativity between pleasure and pain, attacks begun long ago by Burke, and which the hasty theory in question never could have withstood had it not been so useful for purposes of apologetics and edification. The pain of constraint is similarly explained by the escape of energy from the overfed organ into the surrounding tissues, which are incapable of responding to this unusual attack. An impressive corroboration is gleaned in this manner from various fields, and further evidence is found by the deduction of certain æsthetic rules from the

formula. Of these rules the one which there is, perhaps, the greatest present need of inculcating is that which demands the exclusion of pain. "No one who desired to produce an æsthetic work would think of giving it such a form that its appreciation would be dependent upon the holding of one's breath or upon . . . hunger or thirst."* A laborious age that feels the need rather than the presence of æsthetic values is apt to make its art too difficult, and to add so much spice to beauties that they lose their savor. Mr. Marshall, however, is not insensible to the value of discords. He tells us at the end that we should not wish pain out of our lives, because it is a condition of growth and of "elemental partnership in the higher progress of the wider entity."† These last pages are the only place (and none could surely be more fitting) where he gives vent to metaphysical yearnings; indeed, many a reader of philosophical æsthetics will thank him for nothing so much as for the tone and temper of his book, for his simplicity and candor, and for his introduction of a patient and scientific spirit into a realm so largely the prey of whim and dogma.

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Basal Concepts in Philosophy. An Inquiry into Being, Non-being, and Becoming. ALEXANDER T. ORMOND. New York, Charles Scribner's Sons, 1894. pp. viii + 308.

This book is much more an essay at a system of metaphysical philosophy than a psychological treatise. It treats, however, of 'concepts,' and the genesis and nature of concepts are psychological inquiries; while the author admits (p. 300) that all the 'philosophical categories' so called are originally, as it were, psychological. The 'psychological categories' must be 'translated from subjective to objective universals' by being passed through the historic medium and embodied in the experience of humanity.

Professor Ormond starts out with what he calls 'the Norm,' or category which holds the supreme content of man's reflective thinking; and which he finds to be the concept of the Logos understood as conscious personal self-activity. In successive chapters he traces the development of the 'moments' of this concept, beginning with 'Being and Non-being,' and then passing on to 'Becoming,' 'Space and Time,' 'Nature' (Cosmic, Organic, and Psychic), 'Consciousness,' and 'Morality,' in a manner not without plain suggestions of the Hegelian dialectic. Chapters on History, Religion, Art, Knowledge, Logos, God, and Spiritual Activity follow, showing how comprehensive is the

* p. 307. † p. 359.

range of philosophical subjects which the author aims at least briefly to cover.

The distinctive feature of the entire volume is the treatment given to the concept for which the term 'Non-being' is employed. This attempt to establish a better theodicy by a readjustment of this category may be best described in Professor Ormond's own words (p. 302). "It may be maintained with Hegel that the highest category is an absolute idea which comprehends the dual moments, being and non-being, within itself. To this we may yield a qualified assent, provided this idea be translated into spirit and its dialectic be conceived as on its affirmative side, self-affirmation, but on its negative side the denial of its opposite. The reform in Hegelism which has been urged throughout this inquiry may be expressed in the following statement: Being must be identified with spirit. The inner movement of spirit is a dual dialectic in which spirit asserts itself and denies its opposite. The dual movement is thus immanent in being. But the negative which spirit denies is not in being. It is an oppositive excluded conception, which spirit ever wars against and suppresses, but which never passes into its opposite."

We have the greatest respect for all serious attempts to sound the abstruser problems of philosophy to their lowest depths. We disclaim all fear and dislike of metaphysics, but rather wish to see it more diligently cultivated in the future as the only cure for its own worst ailments, the only means of supplying its obvious defects in the past. And we give Professor Ormond respectful credit for his serious and patient essay in metaphysical thinking. At the same time we are more than ever before persuaded that concepts are nothing and can do nothing; can neither, of themselves, count nor account for anything. They are all only mental representations that result from the combined activity of imagination and intellect, and must correspond to actual and concrete existences either immediately known or inferred. Now 'non-being' is no such concept; it is simply a word standing for the refusal to think, or the limit of thinking. It can therefore not be 'warred against,' or 'suppressed,' or constitute one 'moment' in a dual dialectic. Much less can it account for the existence of any form of evil, or constitute either a background, or a barrier, or a reactionary opposite to the will of God. G. T. L.

Inductive Psychology. E. A. KIRKPATRICK, Instructor in Psychology in the State Normal School, Winona, Minn. Jones and Kroeger. Pp. 104.

This little work is an introduction to the study of the intellectual powers by the inductive method. It consists of descriptions, examples,

and questions primarily used by the author in his own instruction, but designed also to enable teachers and others "to pursue the subject understandingly either in books or in daily life, and in the school-room." In the belief that the 'thoroughly-dried specimens' of the ordinary text-book obscure rather than elucidate psychological problems, the attempt is made to lead the student through his own inquiries to a clear comprehension of mental phenomena. Mr. Kirkpatrick writes from the standpoint of recent psychological theory, and is very successful in his introduction of the new results in connection with elementary instruction. It is inevitable, however, that so brief a sketch should include not a few conclusions which more detailed discussion would find open to question; and it is to be regretted that some phenomena deemed fundamental by most writers have received so meagre consideration. A discussion of attention with almost no reference to the influence of feeling, of perception with but a bare sentence or two on localization, of the representative faculties with time-perception unexplained, seems hardly sufficient for even an elementary work. The examples or 'specimens' for study show a similar blending of success and failure. Many are very happy; others will hardly replace those now current.

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La fatigue intellectuelle et physique. A. MOSSO. French translation by P. LANGLOIS. Paris, Alcan, 1894. Pp. 192.

In this popular book Mosso gives a lively and interesting description of a number of experiments and observations which he has made on fatigue. Part of his book is purely anecdotal, and need not be dwelt upon. He has studied the fatigue of birds (quails which have come into Italy from Africa across the Mediterranean, and carrier-pigeons), of sailors, of soldiers, politicians, professors, etc., by means of the ergograph, an instrument invented by him and described in this book. The results given are a popular statement of earlier papers published in various journals.

Les caractères. F. PAULHAN. Paris, Alcan, 1894. Pp. 240.

It is singular that all the studies of character which have been published heretofore, at least to my knowledge, have been *a priori* studies, mainly devoted to classification. To study character has been to classify the types of character ordinarily distinguished or classed by preconceived ideas. This work of Paulhan is no exception to this rule. He applies to character the idea already developed in his work

on the systematization of states of consciousness. At the end of his book, it is true, he seeks a sort of experimental verification of his conceptions by a study of the character of Flaubert; but this is so vague, so poorly documented, that it would serve any other theory as well. But, criticism aside, this new book of Paulhan contains the keen analysis and breadth of view which we expect from him. We can only give a general idea of his classification. In the first part he passes in review the types produced by the predominance of a particular form of mental activity, i.e., the different forms of psychological associations (eleven types, from the 'balanced' to the 'completely unbalanced'); then the types which arise from the kind of tendencies involved (again eleven types).

In the second part the classification proceeds upon the nature of the object of the tendencies, and the types fall into two groups: those resting upon 'vital' tendencies and those upon 'social' tendencies, the former including the gluttonous, the sexual, and all such temperaments in which visual, auditory, gustative, etc., sensations are prominent. Upon the social tendencies are built up the egoistic and altruistic types, all exaggerations of love, family affection, social and professional tastes, etc. The pages devoted to descriptive work are curious and pleasant reading; but we are forced to confess that we prefer more exact observations.

L'éducation de la volonté. J. PAYOT. Paris, Alcan, 1894. Pp. 276.

This book has a beautiful title—one which an aged physician, an aged educator, an aged priest, who had seen and confessed many people, might write entertainingly upon. But M. Payot is a young man, though he has meditated much and mainly upon himself. As a piece of self-examination, however, the book has value. Indeed, it is much to be desired that many such accounts might be written by persons who are capable of recording the manner of their own bringing up, provided they be not simply moral tales, but precise and practical *psychological sources* from which we may draw lessons in personal conduct.

M. Payot busies himself especially with the growth of the voluntary faculty. He holds that we have a certain power of arrest over our ideas—by making appeal to actual sensations, by going through a regimen of voluntary movement, or by using our organs of speech voluntarily for such a purpose. Against our life of feeling, however, we are more poorly armed. He suggests the possible use of simple medicines as a resource against the onset of emotion, but thinks the source of our weakness in this respect is our loss of time by tempo-

rizing. We ought, on one hand, to cultivate associations inimical to the sentiment we wish to dispel, and, on the other hand, to suppress it by inhibiting its expression, by avoiding occasions of its excitement, and by abstaining from its recall in memory.

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Brain Surgery. M. ALLEN STARR. New York, William Wood & Co., 1893. Pp. xii + 295.

To the general reader, Dr. Starr's book is of interest as a presentation of the practical results in medicine and surgery of the vast amount of research of the past thirty years in the field of cerebral localization, and it must be confessed that the impression left after a review of the facts given is rather disappointing. The promises of physiology have certainly not borne fruit to the extent that might have been expected, nor is this lack apparently to be laid entirely at the door of shortcomings in operative technique, for it would appear that in the last few years cranial surgery has been brought to a very considerable degree of perfection.

The opening chapter of the work in question is a general discussion of the diagnosis and localization of cerebral disease, and to the uncritical reader might prove misleading, for from the delightfully brief, lucid, and positive statement regarding the localization of function one would get no hint of the actually very uncertain state of our knowledge on this important point. Such discussion is perhaps only incidental to the real scope of the book, and the fault is a small one, but surely here, if anywhere, dogmatism should be avoided.

To each of the pathological conditions which in his opinion may indicate operation Dr. Starr devotes a chapter. These states are: epilepsy, imbecility due to microcephalus, cerebral hemorrhage, abscess, tumor of the brain, intracranial pressure, and insanity. Of these the results are perhaps most encouraging in the case of tumors of the brain and epilepsy. Of the former, without going into details, the author estimates from a comparison of reports that seven per cent are removable by operation, and from his table of results one finds that of ninety-seven operations recorded, the tumor was successfully removed and recovery ensued in forty-two cases. Regarding operative success in epilepsy, of the forty-two cases collected by Dr. Starr the results were—thirteen cured, eleven improved, fifteen not improved, three died. It must be remarked here that only those cases are regarded as indicating operation where there are distinct symptoms of a localized lesion, which cases would, of course, represent but a very small proportion of the total number of epileptics. On the other hand, the average mor-

tality is but seven per cent for all cases of trephining for epilepsy, which, considering the unfortunate condition of the patients before operation, would seem to justify surgical interference, even with the above results in view.

Of the other pathological states discussed, space does not permit description, but it may be said that the results are generally less successful than those mentioned above.

Notwithstanding the discouraging state of the question at the present time, it is quite possible that with increased knowledge of cerebral physiology and consequent certainty of diagnosis and localization the results will improve; but for this one can only wait. Of matters at present, 'Brain Surgery' may be recommended as a very fair and judicious review.

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THE NERVOUS SYSTEM.

The Effect of Stimulation and of Changes in Temperature upon the Irritability and Conductivity of Nerve-fibres. WM. H. HOWELL. (With the assistance of Mr. S. P. BUDGETT and Mr. ED. LEONARD.) Journ. of Physiology, vol. XVI. Nos. 3 and 4.

In view of the uncertain character of our information regarding the nervous impulse, the papers which deal with its modifications are of great interest. In many cases we are able to separate irritability from conductivity in the nerve-fibres, but find, as might be expected, that in the main they vary together.

This paper deals mainly with the question of conductivity. In general, low temperatures diminish conductivity. High temperatures increase it, first rapidly, then slowly, and somewhere about 50° C. this increase gives place to a rapid decrease which leads to its complete abolition. As a rule, return to the normal temperature brings about a rapid recovery, provided that in the case of high temperature it has not been maintained for too long a time.

In the experiments under discussion, a tube carrying fluid, cold or heated, almost encircled the nerve, the temperature of which was controlled by that of the fluid in the tube. The classes of fibres examined, were the motor, the inhibitory fibres of the heart, the vaso-motor fibres, the sweat-fibres, the afferent (respiratory) fibres, of the vagus, and the afferent fibres of the vaso-motor centres. It was observed that cooling the vagus nerve in the rabbit to 15° C. or below very

often prevented from passing the impulses which would normally stop the heart. In dogs the vagus might be cooled much more before yielding this reaction. In a dog the vagus nerve at 3° C. conducted impulses which weakened the force of the heart-beat, without affecting the rhythm.

In working with the vaso-motor nerves, it appeared that with changes of temperature the vaso-constrictor fibres lost their irritability sooner than the vaso-dilators.

The interesting feature that these observations have in common is the following: that the nerve-impulse passing a point which is modified by temperature takes on a new character, dependent upon the modification; that is, if it passes a part of the nerve which has been cooled, it is decreased in its efficiency despite the comparatively short part of the nerve which has been modified. In the same way, if it passes a heated portion of the nerve it may become increased in efficiency despite the fact that the remainder of the nerve is in its usual condition. Remarkable, therefore, is the influence of this small modified portion of the pathway on the final effect of the impulse. It is also of interest to note that groups of fibres having different physiological functions react dissimilarly under these conditions.

The susceptibility of nerve-fibres to loss of irritability at the point of stimulation was also investigated. In general, nerves are not susceptible to 'stimulation-fatigue.' Stimulation of the sweat and vaso-constrictor fibres in the sciatic nerves of the cat shows, on the contrary, that the reaction disappeared when the stimulation was continued at one point for even a moderate length of time. That this is due to fatigue at the point of stimulation, is suggested by the fact that moving the electrodes even a millimetre towards the periphery produces the initial reaction in full force. Curious reactions were found upon stimulation of the fibres, causing dilation of the pupil. If the cervical sympathetic, including the fibres controlling the pupil, is stimulated on the central side of a 'block,' the stimulation may continue for as long as 20 minutes; yet upon then removing the 'block' the pupil reacts. This is taken to indicate that a 'stimulation-fatigue' does not occur. When, however, no 'block' is introduced, and the nerve is then stimulated, the pupil first enlarges, but soon begins to diminish, gradually returning towards its normal size despite the continuance of the stimulus, showing therefore complete fatigue. When, however, the electrodes are now shifted to a new point on the nerve, a new dilation occurs, a result which is interesting and difficult to explain, in view of the fact that the first experiment indicates an absence of 'stimulation-fatigue' of the nerve.

The Sensory Motor Functions of the Central Convolutions of the Cerebral Cortex. F. W. MOTT. The Journ. of Physiology, vol. xv. Jan. 1894.

To those who have considered the anatomical bases on which the physiological reactions of the cerebral cortex rest, nothing could have been plainer than the fact that all parts of the cortical mass must be both sensory and motor, if these terms are used with sufficient looseness. We have clearly marked in the cortex a locality in which impulses pass from one nerve-cell to another, and in which the direction of the impulse is changed, for incoming impulses arriving at the cortex travel from the cord towards the brain, whereas those impulses on which the motor reactions depend necessarily pass from the brain towards the cord. This being the case, it was anticipated that injury to the cerebral cortex would give rise to disturbances both of motion and sensation in the opposite half of the body. The disturbance of motion has been comparatively easy to detect; but disturbance of sensation was both more difficult to determine and less constant. Special localities for the dermal sensations have been proposed; and on the basis of experiment, Schäfer and Horsley suggested that the gyrus fornicatus was their centre; but there are good reasons for questioning this result.

So far as the work of Mott is concerned, he inclines to place the termination of the afferent fibres in very nearly the same location as that occupied by the cell-bodies which give rise to the efferent fibres. From the nature of the case the efferent fibres by means of their collaterals may have different terminations, and even in a given locality terminate in a diffuse manner; and, by consequence, very limited lesions which may impair motality do not necessarily impair sensibility to the same degree. Mott's contribution to the subject is briefly this: Monkeys very carefully selected, on account of their sensibility, were operated upon in such a way that the connection of a motor area was severed at some distance below the surface, the severed portion being allowed to remain in place. Loss of motion and loss of sensation were found in the same regions, at the periphery; and the conclusion is drawn that the so-called motor regions are nearly coincident with the localities in which the fibres for the dermal sensations from these parts terminate in the cortex.

The Derivation of the Pineal Eye. Preliminary Announcement. WM. A. LACY. Anatom. Anz., IX. B. 5, 6. 1894.

Within the past seven years a large number of studies have been made on the organ now known as the pineal eye. This small structure

is situated in the middle of the head just above the mid-brain, and in some forms of amphibia and reptiles can be clearly seen from the outside. Through the work of de Graaf (1886) it came to be generally accepted as an eye, constructed on the plan found among the invertebrates. Since that time its interpretation as a visual sense-organ, its origin, its connections with the brain, and its functional condition, have been widely discussed.

The observations of Lacy deal with its origin. In the Elasmobranch Fish (*Squalus acanthias*) he has observed in the neural plate, behind the pair of depressions which later form the lateral eyes, two other pairs of depressions similar in character. These two pairs become associated with the mid-brain when the neural tube is formed. The posterior pair disappears, but the anterior persists. The latter forms parts of the roof of the mid-brain, and marks the locality at which the epiphysis grows out. In Squalus the pineal eye is not distinctly differentiated, but the enlarged end of the epiphysis may be homologized with it.

The argument is therefore made as follows: The epiphysis originates from paired structures which in the first instance are comparable with the optic depressions giving rise to the lateral eyes, and so the final modification of the tip of the epiphysis into a visual sense-organ would be in harmony with its early embryonic history. The paired origin appears also to explain the fact that the pineal eye and epiphysis appear in some forms to be distinct structures.

These observations are full of suggestion, and the next step, as the author points out, is to determine whether in other animals the pineal outgrowths have a similar embryonic history. H. H. D.

ANTHROPOLOGICAL.

The New Life: A Study of Regeneration. ARTHUR H. DANIELLS. Am. Jour. of Psychol., VI. 61–103. Oct. 1893.

Mr. Daniells, with many other thoughtful theologians, believes that theology has "lapsed from its high functions," and has "become so conventionalized and rigid that religion itself has often fallen into disrepute and is losing its hold both upon the masses and the cultured classes." But he believes that anthropology, "which is the pedagogical root and very lifespring of theology," has itself entered upon a new stage of development and is now ready "to offer a few ripe insights for the rehabilitation of theology, point by point." "As an illustration of what is thus promised to theology and religion," Mr. Daniells has made a very careful study of the phenomena of puberty, viewed

from the anthropological, physiological, and psychological points of view, and endeavors thus to throw light upon the theological doctrine of regeneration. He tries to show "that . . . regeneration is one of the deepest needs of the human body and soul felt among savage and civilized men of all races and times." He attempts "to rebase the doctrine on sound anthropological and psychological principles," hoping thus to "strengthen the pulpit and . . . widen and deepen the sympathy between theology and other sciences."

In pages 63–79 we have a very interesting account of the initiation ceremonies at puberty as practised by sundry peoples, and an attempt is made to point out the significance of some of these ceremonies to those that use them. This shows wide acquaintance with the literature of the subject and is to my mind by far the most valuable part of Mr. Daniells' work. Yet if it is upon the evidence here presented that Mr. Daniells relies to show that regeneration of body and soul is felt as a need by savage and civilized races alike, I cannot see that he has proved his point. It shows indeed that the physical and mental changes of puberty, which might perhaps be termed a regeneration of body and mind, are recognized as a fact by nearly all races. It is possible also to speak of the transition from childhood to manhood as an entrance into a new life. But this is not what the theologian means by regeneration and the new birth, and the recognition of the one most certainly does not prove the recognition of the other.

The physical and psychical phenomena of puberty and adolescence are outlined pp. 79–93. Especial emphasis is laid upon the fact that the obvious development of the sexual impulses is only one aspect of a profound and rapid modification of the whole psychical nature. The intellectual life quickens, the ethical instincts become stronger, the emotional life becomes deeper and more varied.

In this connection Mr. Daniells brings in the thesis which he wishes to prove: "that during the adolescent years there is a necessity grounded in man's nature as a human being, of a new consciousness of man's relation to God, of the conscious choice of Christ as the chief object of his love and service, with an overmastering sense of the duties and responsibilities involved in this ideal life, in order that he may not only be saved from selfishness and the dangers which threaten youth, but also that his newly-awakened capacities and powers may be controlled and used for the development of the noblest character."

The burden of this thesis turns upon the value which we ascribe to the word 'necessity.' If Mr. Daniells merely wishes to say that the acceptance of the Christian scheme is *essential* to the development of the highest type of character, he will find many to agree with him.

But he has not, as far as I can see, adduced any proof of the statement, nor has he shown that such acceptance is based upon anthropological or psychological principles. The period of doubt incident to the intellectual growth of adolescence may indeed end in Christian faith, but it may also end in rejection of that faith, and it does not lie within the scope of either anthropology or psychology to show which is the normal and right end, but of philosophy.

But this is not, I think, what Mr. Daniells means. He wishes to show that the psychical change described by theologians as regeneration, conversion, the new birth, etc., is simply a stage in the normal development of the human soul, a profound psychical modification which usually supervenes at puberty or shortly after and is therefore to be co-ordinated with the numerous other psychical and other changes of the same period. If this be true, he has succeeded in putting the doctrine of regeneration upon a psychological foundation. Mr. Daniells shows us that one is more susceptible to religious impressions in adolescence than at any other period of life, and that the spiritual life, if it is to awaken at all, will probably awaken then. But he does not even attempt to show what is essential to his theory as I conceive it, that this awakening to religious things is natural, i.e. universal, in the same way in which the other changes of puberty are natural. Until that is established the doctrine of regeneration is as far from having a sound psychological foundation as it ever was.

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CHILD-PSYCHOLOGY.

Mental Defect and Disorder from the Teacher's Point of View. JOSIAH ROYCE. Educational Review, vol. VI, 1893, pp. 209, 322, 449.

The author, writing not as physician, but as 'student of human nature,' considers "in a practical way . . . the attitude that a teacher ought to be ready to take toward . . . cases which lie . . . in that wide and ill-defined border-land region which separates the world of the sane from the wilderness of insanity." "What a studious layman can do in dealing with nervously and mentally defective pupils is to supplement his ordinary knowledge of human nature by such psychological insight and reading as will make him keen in watching for the symptoms when he meets them and apt to judge a burdened pupil in a psychological rather than in a merely censorious or moralizing spirit." The teacher may, moreover, learn much that is interesting and important from a study of defectives. "It is in fact the mission of the mentally defective to give us . . . object lessons in psychology."

A condensed prefatory statement of the chief forms of normal conscious activity from the standpoint of 'the empirical psychologist as such,' is further condensed in the statement: "What is all the while going on is the adjustment of this organism to this environment under the influence of these stimuli, and by virtue of these central dispositions or habits,—the adjustment always expressing itself by these motor responses and inhibitions which constitute our more or less highly intelligent conduct." Further: "Our perceptions, imaginations, general ideas, impulses, trains of thought, and lines of conduct are modified and determined, not only by our past habits, but also by the current state of nutrition or of exhaustion, or in general by the transient psychological susceptibility of our centres." "The actual states of nutrition or exhaustion of our nervous centres," as well as various specific and organic sensations, "are more or less immediately represented in our current emotional condition."

The test of the normality of a mental process is "the success of the adjustment of the organism to the environment in so far as this success depends upon the processes that embody the habitual functions of the highest or mental grade of nervous centres themselves" ("slightly modified from Dr. Charles Mercier, *Sanity and Insanity.*") Confining himself to 'border-land cases,' the author considers first those arising in childhood. The hallucinations of childhood may be unimportant, but if they become systematized and recurrent, should be carefully watched, especially if accompanied by other signs of abnormality, as tendency to grow easily delirious, pessimistic moods, very active and precocious wits, symptoms of general physical inability. As to bad nervous heredity: "Respect heredity, but never despair on account of it, since its caprices are endless." In youth 'definable insanities become more common.' The most frequent types of the 'border-land cases' are: (1) Emotional depressions and exaltations, characterized by the author at some length; (2) Emotional dullness; (3) Mental confusion and failing of memory; (4) Lassitude of will; (5) Painful insistent ideas.

A difficult and important problem is to distinguish between 'the normally constituted person making bad blunders' and 'the sick man in need of treatment.' The teacher should study these cases as a 'true naturalist,' as a biologist studies cell-growth, etc.; must love the study; must be 'tender with the sanctities of youthful feeling,' treating the burdened pupil as a 'beautiful wounded bird.' The teacher should analyze 'the successive grades of mental facts,' as sensations, elementary feelings, perceptions, etc., and seek to place the defect in this series. In estimating the importance of defects one must determine

how far they affect one's actual habits of conduct and how stubbornly they refuse to yield to 'gentle social suggestion.' An intellectual or moral abnormality unaccompanied with feelings of suffering is more grave on that account.

The author promises another article on anomalies and abnormalities of temperament. In view of the fact that these articles are addressed to those who are almost never medical experts and rarely psychological experts, it is to be wished that the forthcoming article should contain more such clear and explicit characterizations and advices as those given by the author in treating of Emotional Disorders. The fact that 'there is no magic about the teachers' art of treating mental defects' except 'humanity, experience, watchfulness, intelligent comprehension of the actual situation—and tact' does not make such explicit characterizations and advices superfluous.

Die geistige Entwickelung in der ersten Kindheit, nebst Anweisungen für Eltern. W. PREYER. Berlin, 1893. Pp. 201.

Mental Development in the Child. W. PREYER. Tr. by H. W. BROWN. International Educational Series. New York, Appleton, 1893. Pp. 170.

Professor Preyer's object is to introduce mothers to methods of observing children. The author's *Seele des Kindes* (3d Ed., Leipzig, 1890) is so well known (even to the general educational public, through Mr. Brown's translation of the second edition) that a review of its contents is superfluous in this journal. The book will doubtless receive the wide reading it deserves, and will contribute materially to the host of influences that are making toward a scientific psychogenesis. It is to be regretted that the translator has not given to American readers the sixty-page "Anleitung zur Führung eines Tagebuches über die geistige Entwickelung kleiner Kinder, etc.," which concludes Preyer's volume.

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Notes on the Development of a Child. MILLICENT W. SHINN. University of California Studies. Berkeley, published by the University, 1893. pp. 88.

This is the first instalment of a work which will prove exceedingly helpful to psychology. Miss Shinn has carried on a careful, personal, daily study of *one child* (a healthy, bright California girl) during the first three years of her life, and recorded her observations on the spot. The present volume is confined to the study of Sight, and the observations are arranged topically, in a manner similar to that of Preyer,

though with some additional topics. The sections on Color Perception and Appreciation of Form are of special interest. Some of the child's first attempts at drawing are reproduced in full-page cuts. A very welcome section is that devoted to the development of the child's Interest in seeing. Careful study of this and kindred topics is much needed. The author is very diffident about putting forward theories, but she is certainly doing her part towards furnishing child-psychology with that which it needs most at present, viz., 'a body of carefully observed facts' (to quote the words of Prof. Le Conte in the Introduction). We look forward to the appearance of the remaining portions of the work. FREDERICK TRACY.

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L'imagination et ses variétés chez l'enfant. F. QUEYRAT. Paris, Alcan, 1893. Pp. 162.

In this book M. Queyrat presents in a popular way, as he says, for teachers, the evidence recently brought out of 'types' of memory and imagination—visual, auditive, motor, etc. He makes no effort to explain the facts or to add to our knowledge, and his information is drawn largely from French sources. Some of the newer analyses, also, have escaped him altogether. But for the audience which he addresses the book is well suited, and may be commended to teachers and students as a clear statement of a class of psychological results likely to prove of very great pedagogical importance. J. M. B.

VISION.

(1) *Experimentelle Untersuchungen über die Helligkeit der Farben.* EDUARD GRUBER. Philosophische Studien, IX. 429–446. 1893.

(2) *Untersuchungen über den Lichtsinn.* KARL PÉTRÉN. Skand. Archiv für Physiologie, IV. 420–447. 1893.

(3) *Zur Lehre von den Gesichtsempfindungen welche aus successiven Reizen resultiren.* KARL MARBE. Phil. Studien, IX. 384–400. 1893.

(4) *The Use of the Rotating Sectored Disk in Photometry.* ERVIN S. FERRY. The Physical Review, I. 338–345. 1894.

(5) *Ueber den kleinsten Gesichtswinkel.* E. A. WÜLFING. Ztschr. f. Biologie, XXIX. N. F. XI. 199.

(1) Gruber finds that he is able to determine the relative brightness of two different colors, or of a color and a given gray, with a mean error which is hardly perceptibly greater than when the comparison is

made between two sensations of the same color-tone. This is contrary to the experience of Helmholtz and other writers on the subject. He goes on to discuss the belief of Hering and of Hillebrand that the effect of the addition of the specific color-process to the accompanying white-process of a mixed sensation is, in the case of blue and green, to diminish the brightness, and in the case of red and yellow to increase it. Hillebrand's argument is based upon the fact that, if a gray, a yellow, and a blue are chosen, which are equally bright when the illumination is so faint that no color is perceptible, then in the illumination of ordinary daylight the blue will be darker than the gray and the yellow will be brighter than the gray. (That the yellow is much brighter than the blue is the well-known Purkinje phenomenon, but that the gray is intermediate between them was not mentioned by Purkinje.) That the specific blue-process exercises an actual darkening effect upon what would be the brightness of the gray-process if it existed by itself, Gruber considers is not proved by this fact, and for this reason: the fact can be perfectly well explained by the assumption that the relative increase of the color-component and the gray-component is different for different wave-lengths; and this assumption is in fact the more probable, because, first, there is no reason to suppose that they would be the same, and secondly, increase of illumination beyond a certain point increases the gray component more than the color-component for all wave-lengths. It is difficult to see that either of these considerations adds anything to the fact that, in the absence of information, one supposition is as reasonable as another. But if they do, they have a bearing only upon the fact that blue becomes less bright than yellow with increased illumination, and no bearing whatever upon the fact that blue becomes less bright than gray. Hillebrand's argument is indeed fallacious, but not at all more so than Gruber's argument against it. Gruber also found that to a red-green blind person a given green pigment looked no brighter (that is, was matched with no brighter gray) than to the normal-eyed. But it does not appear that any pains was taken to find a green that was really neutral to the individual tested; most greens are either yellow or blue to those who see blue and yellow only, and their neutral band is very narrow (for this individual near *E*). The experiment is therefore of no value, but it suggests an interesting field for farther investigation.

(2) Petrèn finds that, for most experiments on the sensation of light, it is extremely desirable that the background should be neither black nor white, but an even gray; the difficulty of securing correct adaptation is then avoided, for this is the illumination to which the eye is adapted in ordinary use. With this background, and with ob-

jects, both black and white, which subtended a visual angle of 50′, he found that the period of excitation necessary to produce a sensation was $\frac{1}{1000}$ of a second. The sensitiveness to a change in the period of excitation is slight for less periods than this; for greater periods, it first rises, and then remains constant. One twelfth was the smallest difference perceptible. The time necessary for the production of the maximum effect of a light-sensation cannot be exactly determined, but it is not less than $\frac{3}{10}$ of a second. The most favorable visual angle for the rapid perception of form is 35′, or between 23′ and 53′.

(3) The first experiments upon this subject were made by Plateau and by Emsmann. They found that different conditions were necessary for the fusing of different colors, but as it does not appear that they were careful to obtain colored papers of equal saturation and equal brightness, their experiments on this head are not conclusive. But it is certain, on the other hand, that the less the intensity of the illumination, the greater may be the duration of the separate impressions,—that is, with sectors of a given size, the less is the requisite velocity of rotation. Marbe has confirmed this result, using a large number of exactly determined intensities and periods. He finds, however, that the two quantities do not vary in a simple proportion. When the two sectors were of unequal size (the experiments were made only in black and white) he found that with increasing intensity the differences of the two sensations could be increased, and so with diminished velocity of rotation, other things, of course, being equal. Fusion takes place more readily when the white sector is larger than the black one, than when the reverse is the case. The view of Plateau and v. Helmholtz that, provided the number of the sectors is the same, the relative size of the black and white sectors is of no consequence, is thus found to be not correct.

(4) It is commonly assumed that when a rapidly rotating disk from which radial sectors have been removed is used to diminish the intensity of light, the proportion of light cut off is equal to the ratio of the aperture of the disk to the entire disk. While this is of course true physically, it has been found to be not the case for sensation; the error which arises is negligible only when the total aperture is more than one half of the entire disk. The error depends only upon the total aperture, and not upon the number of the vacant sectors which go to make it up. With an aperture of 24°, white light was diminished from ten to fifteen per cent more than theory would require. The error is not a function of the total amount of illumination. The writer considers that it can be explained in the following way: The luminosity of a given white light is the sum of the luminosities of the different wave-lengths which

compose it. Both the intensity of the retinal sensation and the time light must act upon the retina in order that it may be seen are direct functions of the luminosity. When light acts upon the retina for a very short time the elements of low intensity—i.e., at the ends of the spectrum—will not produce their maximum impression, and hence the total luminosity will be diminished. But it was noticed that when the disk rotated so slowly as to produce flickering, *more* light went through the screen than should go through theoretically, and this the author attributes to the building up of separate impressions upon the retina. Why the separate impressions should be built up in the one case and not in the other he does not explain. When the error was very marked (with rapid rotation) the light at the disk end of the photometer was of a bluish tint. How this shows that blue light—that is, the non-luminous end of the spectrum—was disproportionately cut off does not appear. Other experiments were made in confirmation of the fact that in the rotation of black and white disks the luminosity is the same as it would be if the light emitted were distributed evenly over the whole disk; monochromatic light was used throughout a wide range of luminosity and wave-length.

(5) The author points out that the separation of luminous points is a process not well adapted to the detection of the *minimum visibile*, on account of the necessary errors caused by irradiation. He made use of a vertical line whose lower half could be moved by a measurable amount to the right or to the left. He found that a deviation of .1 mm at a distance of two metres could still be detected, which would correspond to a least visual angle of ten seconds. This can be brought into harmony with the known size of the cones in the fovea only upon the supposition that the cones are not separated from each other by a distance greater than their diameter.

C. L. FRANKLIN.

BALTIMORE.

Flatternde Herzen. ADOLF SZILI. Zeitsch. f. Psychol., III. 359–387. 1891.

Zur Theorie der 'flatternden Herzen.' A. SCHAPRINGER. Zeitsch. f. Psychol., v. 385–396. 1893.

When patterns or figures of certain colors occur on grounds of certain other colors, the red or yellow parts seem to stand out in front of the plane of the blue or green parts, and to form an independent lattice-work. If now the eyes of the observer move about, an apparent movement or flickering of this lattice-work over its ground takes place because of the absence of the parallactic excursion which would be

made by an actual lattice-work in front of the observed plane. The same is true if the observed object itself be moved back and forth with some rapidity.

The first of the above papers gives the results of a careful investigation of these phenomena and the circumstances which favor their occurrence. The second paper furnishes an explanation for them. Helmholtz' explanation is, that the light-impression in the eye does not arise and disappear with the same rapidity for the different colors, and therefore the blue remains apparently somewhat behind the red (Phys. Op. $^{1}383$; $^{2}533$). Schapringer accepts chromatic aberration as a partial explanation. Differently-colored rays of light are not equally refracted by the lens ; if blue rays are brought to a focus on the retina, red rays from an equally distant object have their focus behind the retina. To focus the latter on the retina requires a change of accommodation which arouses the impression that the red is nearer than the blue.

But this cannot be a complete explanation, for to some people the blue appears nearer than the red, or at the same distance, or sometimes nearer sometimes farther. These differences are accounted for by the introduction of a second factor already noticed by Einthoven, but not applied to these phenomena; namely, the non-coincidence of the point where the line of regard passes through the plane of the pupil with the central point of the pupil. Fixate and sharply accommodate for a blue point, and a red point which lies close to and directly above it will produce on the retina a dispersed image whose central point will lie not directly under the image of the blue point, but to right or left of it. This difference does not exist at the retinal pole of the eye's axis, but grows continually greater the farther we go peripherally from this point. Now whereas on a moving surface the red and the blue have the same rapidity, in the retinal image the red moves faster than the blue. When an image moves peripherally from the fovea centralis, in one eye it approaches, in the other recedes from, the retinal pole of the axis of the eye ; so that the increasing mutual distance of the colors in the one eye is balanced by the decreasing distance in the other, and the flickering does not occur. But having passed beyond this pole the distance increases in both eyes, and more rapidly as the image progresses farther towards the periphery ; and this fact together with the greater sensitiveness of the peripheral parts for the perception of slight movements (Exner) accounts for the greater distinctness of these phenomena in indirect vision. That they are more apparent in dim light is due to the widening of the pupil, which causes its central point to deviate still more from the line of regard.

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BROWN UNIVERSITY.

L'audit'on colorée chez les aveugles. JEAN PHILIPPE. (Details to be printed in France.)

Pseudo-chromæsthesia in its different forms is more common in the blind than in others. Thus the phenomena were discovered in 30 cases of the 150 examined. This greater frequency results from the general tendency of the blind to associate their lost visual memories with the auditory and other sensations which have taken the place of sight. The phenomena vary greatly according to the occupation of the blind. They are more frequent in those who are versed in music and literature, especially in the former. No instance of pseudo-chromæsthesia was found in those born blind. In all other cases it may be present, and even seems to depend on the blindness, for it is usually subsequent and sometimes follows its variations.

AUTHOR'S ABSTRACT.

SORBONNE, PARIS.

HEARING.

Researches in Acoustics. ALFRED M. MAYER. Am. Journ. of Science, vol. XLVII. 1–28. Jan. 1894.

The first investigation in this paper is on "the law connecting the pitch of a sound with the duration of its residual sensation," and is in continuation of researches published in the above journal in 1874 and 1875. More especially the object of this research was to measure the "duration in which the after-sensation of a sound does not perceptibly diminish in intensity." Three forms of apparatus were used, in all of which the continuous sound from a tuning-fork reinforced by a resonator was cut off into periods of a given length by a perforated revolving disk. Most trustworthy results were given where the ratio of the diameter of the hole in the emitting end of the resonator to that of each hole in the disk was 1 to 2. Taking the period between maximum and minimum intensities of succeeding sound-pulses at the point where the pulses became continuous, the author found that the residual sensation diminished in duration as the number of vibrations increased: being, for instance, 0.0361 sec. for 64 v. d. and 0.0049 sec. for 1024 v. d. Expressed in vibrations, however, the corresponding values are 1.38 and 3.01. The computed values show a very close agreement with the observed values.

In saying that no one has made similar researches since his first papers published in 1874–75, the author has overlooked Urbantschisch's work 'Ueber das An- und Abklingen akustischer Empfindungen' in Pflüger's Archiv, XXV, 1881, p. 328. Urbantschisch found with our au-

thor a decrease of the residual sensation with increasing number of vibrations.

We infer from the account of the experiments that Prof. Mayer took the point where the interruptions just blend into a continuous tone as the limit for determining the residual effect: this gives a lower value than Urbantschisch found by descending from the blend to the point where the interruptions became just audible. If the ear could stand it, combining the two ways according to the method of minimal changes would be better than either way separately. Whether we may assume that the periods of the rise and decadence of a tone-sensation are equal or not, at any rate it is interesting to observe that the duration of the residual tone-sensation agrees closely with the time found by several observers (Pfaundler, Kohlrausch) for the rise of a tone-excitation into consciousness, i.e., the time of two to three vibrations. (Cf. G. Martius, 'Ueber d. Reactionszeit und Perceptionsdauer d. Klänge,' Phil. Stud. VI. 394.)

The second part of the paper is 'On the Smallest Consonant Intervals among Simple Tones,' as derived from the blending of beats. Dr. Koenig of Paris assisted in this investigation with forks not at the author's command. For tones above 192 v. d. the computed and observed values of the consonant intervals agree closely, but for 48 and 64 v. d. Dr. Koenig found no consonant interval short of the octave. This Prof. Mayer refers to the interference of inferior and superior beats. On the other hand, there was considerable difference between the values found for beats and those calculated from interrupted tones. The factor of the relative intensities of the interrupted and the beating tones may affect the ratio of the results. Besides, as Wundt has pointed out (Phys. Psy., I' S. 474), the blending of interrupted tones is dependent on the size of the spaces between the holes of the rotating disk, as well as on the number of tone-pulses.

In the third part of the paper, which is on 'The Duration of the Residual Sonorous Sensations as deduced from the Smallest Consonant Intervals among Simple Tones,' the author does not find that the discrepancy between the results from interrupted tones and from beats is harmonized by taking the beats during the time between the halves of the maximal amplitude of vibration instead of between the maximal amplitudes themselves. Here, of course, the question of the just observable difference comes up. This value is by no means so simple a function as has been commonly supposed, for it depends not alone on the intensity of the stimulus, *but on its rate of change.* We do not quite understand Prof. Mayer when he says that *a priori* considerations lead Professors Cattell and Fullerton in their work on 'The Percep-

tion of Small Differences' to 'the opinion that it is probable that the sensation is directly as the stimulus.' These investigators, or more especially Prof. Cattell, propose on the basis of their experiments to substitute for Weber's law the following: "The error of observation tends to increase as the square root of the magnitude, the increase being subject to variations whose amount and cause must be determined for each special case" ('Perception of Small Differences,' p. 153).

The suggestions made by the author on a method of determining just-observable differences of intensities of sound by means of the micrometric observation of amplitudes of vibration are valuable. It is a pleasure to find that Prof. Mayer's ability and technical resources are again to be put into use for the benefit of psychological acoustics.

F. ANGELL.

STANFORD UNIVERSITY.

MEMORY.

Experimentelle Beiträge zur Untersuchung des Gedächtnisses. G. E. MÜLLER and F. SCHUMANN. Zeitsch. für Psychol., Bd. VI. 81–190, 257–339. Nov. and Dec. 1893.

Stimulated by the work of Ebbinghaus, the authors set out in 1887 with the general purpose of investigating the extent to which his methods might be employed for further research. Their labors have extended intermittently over a period of five years.

Although containing some rather radical alterations, their general procedure is highly similar to that of Ebbinghaus. The one really fundamental difference is found in the fact that in the present instance the subject in no case knew either the purpose or the result of the experiment, whereas Ebbinghaus, being his own subject, was kept constantly aware of both these things. As in his work, a large number of meaningless syllables were constructed, consisting of a vowel, or diphthong, between two consonants. It was decided, owing to certain preparatory experiments, to prevent the occurrence of alliteration, rhyme, and assonance, together with the too frequent repetition of diphthongs and difficult consonants. These, with a few other less important modifications, were, however, made in accordance with a fixed rule, which allowed chance to be always the deciding factor in the actual arrangement, and never the subjective preference of the experimenter. The syllables thus obtained were then arranged on a drum, which revolved at a medium and uniform rate, exposing the syllables successively through a slit in a screen. [In the experiments of Ebbinghaus the whole series was exposed at once.] The subject then pronounced the syllables

aloud in trochaic rhythm, save in one set where iambic was employed, and the process was continued until the whole series could be correctly anticipated.

The nerve of the whole method lies in the various highly ingenious modifications of these memorized lists, which the subject is then required to learn again, his rapidity in the last instance compared with his rapidity in the first showing at once the effect of some one constant factor. Suppose, for example, that on one day six series of twelve syllables each had been learned. On the following day are learned two series formed exclusively of syllables used the preceding day, but so arranged that no two syllables stand side by side, which previously were in immediate juxtaposition. Two more sets are learned, in each of which six syllables occupy the same positions they did the day before, both with reference to each other and to the trochaic measure, i.e., they bear the same accent. Still a third set of two series is memorized, in each of which six syllables again stand side by side as on the first day; but the position in the foot is now changed, and consequently the accent altered. It will be seen at a glance that each of these sets presents different constant factors. In the first, the syllables themselves alone remain the same. In the second, both the order and the accent are in part identical, while in the last only a portion of the order is retained. The varying ease and rapidity with which these different series are committed to memory will serve as an index of the influence these various factors exert. This illustration must suffice to convey, however inadequately, a general notion of the mode of procedure.

One may summarize the more important results as follows: (1) The suppression of rhythm in the attempt to memorize such lists as these renders the task much more difficult, consuming about twice as much time as otherwise is necessary. For Germans trochaic measure seems to permit more rapid memorizing than iambic, probably because the majority of German dissyllables have the accent on the first syllable. In the pronouncing of such lists there occurs an involuntary chief ictus on certain syllables,—conspicuously on 1, 5, 7,—coupled with a slight pause and lowering of the voice after 6. The result is to split the series ordinarily into two. (2) By means of a pneumograph records were made showing the peculiarities of breathing during the experiments. Each series of twelve syllables shows a double apex in the breathing cuvre, corresponding to the above-mentioned rhythmical division of the series. (3) The syllables first memorized are by no means always those which stand earliest in the series. Often the last half of a list is committed first. That syllable which first attracts the

attention strongly is likely to be first learned, and precisely which that shall be depends upon circumstances purely accidental. An equal distribution of attention over the several members of a series does not occur. (4) As regards the sensorial character of lists thus memorized, the results are tentative and suggestive rather than conclusive. Visual, auditory, and kinæsthetic elements all manifest themselves, and under the influence of practice the form used seems to vary; e.g., from visual to auditory. (5) The first of any two successively—as well as simultaneously—experienced syllables tends to call up the second, and, when associated through intermediate syllables, the association is stronger if both are accented. Furthermore, the second syllable in a trochaic measure tends to call up the first, and apparently this tendency is stronger than the tendency to call up the first syllable of the next succeeding foot. This result is not to be interpreted as proving that every presentation tends to reproduce by association its immediate predecessor, but is rather to be viewed as belonging under the general law, according to which each element of a presentation-complex tends to the reproduction of the whole. Associations of syllables with their numerical positions ('absolute positions') in the series were apparent. The results of this nature seem to be connected with the previously-mentioned division of the syllables by accentuation. Ebbinghaus seems to have overlooked this element. It may properly be added here that his adoption of the decreased effort demanded for re-learning a list, as the criterion of the strength of association between the separate members of such a series, is quite unwarranted, for it disregards at least one important consideration—i.e., the complex nature of the associative tendencies represented by any one syllable. (6) Under some conditions, syllables with already-established associations prove more difficult to memorize in combination with new syllables, than where such associations are lacking. The previous associations press in on consciousness and disturb attention in various ways. (7) The success of any particular attempt to memorize seems to depend in part upon its time-relations to the other attempts of the same day, e.g., whether it comes first or last. We need only say that attention and fatigue are the principal conflicting elements. A gradual improvement in the rapidity of learning by heart was constantly manifested. (8) The varying ease with which lists of this kind are learned depends not wholly upon subjective differences, but in part upon objective conditions valid in some degree for all persons.

The mathematical problems encountered in interpreting the results are discussed with great care and much fulness. The calculation affecting the widest range of considerations is that by which is

obtained a so-called 'central worth' for the number of repetitions necessary to memorize a list. This is simply a quantity attained under conditions intended to eliminate the disturbing effects on the arithmetical average of large individual variations.

Upon one and another point of detail careful readers are sure to find room for critical queries. With a piece of work covering so much ground this could not be otherwise. Professor Ebbinghaus is quite certain to find occasion for reply to some of the respectful strictures upon his work. But taken all in all, the masterly appreciation of the significance of the problems involved, the untiring energy and ingenuity with which they have been attacked, and the admirable discrimination with which the results are interpreted combine to make this one of the most hopeful pieces of recent experimental work. The Old Benchers will proclaim that the definite results are out of all proportion to the time and labor expended. But unless the whole method can be proved fallacious, one must admit a genuine enlargement of our accurate psychological knowledge.

JAMES R. ANGELL.

UNIVERSITY OF MINNESOTA.

Le problème de la conscience du moi. Dr. PAUL CARUS. Paris, Alcan, 1893. Pp. 144.

Dr. Carus discusses the problems of consciousness and self from the monistic standpoint. Starting with the incommensurability of consciousness and physical phenomena, he shows the inadequacy of materialistic and spiritualistic solutions of the difficulty, and urges the superiority of monism over a dualistic interpretation. Objectivity is the outer side of being, of which subjectivity is the inside or kernel.

The author examines at considerable length (chap. VI) the neuroses of reflex and voluntary movement, and their relation to thought. "The mechanism of thought consists in combinations, separations, and recombinations of representative images or symbols;" "the object of thought is adaptation to environment." Passing to the genetic problem, he attributes to the lowest forms of animal life *isolated feelings*, which become states of consciousness "only in organisms where sense-impressions revive memory-traces, and an interaction can take place between different feelings." He distinguishes (chap. IX) *consciousness* from *intelligence*, including under the latter not only conscious states, but also many sense-impressions, movements, and intellectual functions which are not accompanied by consciousness; all these are 'mental operations.' "The unity of consciousness is not an intrinsic property

of mind" : it is the result of concentration (attention). The continuity of man's psychic life is due not to any permanence of substance, but to the 'permanence of form' which characterizes the memory-traces.

The author takes decided issue with the utilitarian theory. He minimizes the value of pleasure as a motive and claims that "pain is an important and even beneficent factor in evolution." Mr. Spencer's state of perfect adjustment between organism and environment is an unrealizable dream. The final chapters treat of immortality and the theistic problem.

H. C. WARREN.

PRINCETON.

NEW BOOKS.

Introduction à la psychologie expérimentale. ALF. BINET. Paris, Alcan, 1894. Pp. 146.

Man and Woman. HAVELOCK ELLIS. New York, imported by Charles Scribner's Sons, 1894. Pp. 409. $1.25.

Éléments de psychologie physiologique et rationnelle. Dr. SURBLED. Paris, Masson, 1894.

Zur Lehre vom Inhalt und Gegenstand der Vorstellungen. K. TWARDOWSKI. Vienna, Hölder, Pp. 111. M. 2.40.

Materials for the Study of Variation treated with Especial Regard to Discontinuity in the Origin of Species. W. BATESON. London and New York, Macmillan & Co., 1894. Pp. xv, 598.

L'eredità e l'origine delle specie. PIETRO MANTIA. Palermo, 1894. Pp. 43.

Psychiatrie. TH. ZIEHEN. Berlin, Friedrich Wreden, 1894.

La famille neuropathique. CH. FÉRÉ. Paris, Alcan, 1894.

La contagion du meurtre. Dr. P. AUBRY. Paris, Alcan, 1894.

Classification objective et subjective des arts, de la littérature, et des sciences. R. DE LA GRASSERIE. Paris, Alcan, 1894.

Hegel's Philosophy of Mind. Translated from the Encyclopædia of the Philosophical Sciences. Oxford, at the Clarendon Press, 1894. Pp. cciv, 202.

A Study in the Origin of German Realism. NORMAN WILDE. Contributions to 'Philosophy, Psychology, and Education,' Vol. I. No. 1. Columbia College, New York. Pp. 77. 60 cents.

Das Ganze der Philosophie und ihr Ende. Ihre Vermächtnisse an die Theologie, Physiologie, Æsthetik und Staatspädagogik. RICH. WAHLE. Vienna, Braumüller, 1894. Pp. xvi, 558. M. 10.

History of Trade Unionism. SIDNEY and BEATRICE WEBB. London and New York, Longmans, Green & Co., 1894. Pp. xvi, 558.

NOTES.

Professor DEWEY has accepted a call to the Head Professorship of Philosophy in the University of Chicago.

Professor FULLERTON has been elected Vice-Provost of the University of Pennsylvania.

Professor EBBINGHAUS, one of the editors of the *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, has left Berlin to occupy a chair in the University of Breslau.

Mr. HENRY RUTGERS MARSHALL has been appointed Honorary Lecturer on Æsthetics and Dr. NORMAN WILDE Assistant in Philosophy in Columbia College.

MR. JAMES R. ANGELL has been appointed Instructor in Psychology in the University of Chicago.

The death of Professor GEORGE JOHN ROMANES, which occurred at Oxford on May 23, is a serious loss to psychology as well as to biology. Romanes was born in Canada on May 20, 1848, and was consequently just forty-six years old. Had his life been longer other equally important works from his hand would undoubtedly have followed *Animal Intelligence*, *Mental Evolution in Animals*, *Mental Evolution in Man*, and his other writings. These, however, suffice to give him a high place among the great men who have developed the doctrine of evolution.

The *Philosophical Remains* of G. CROOM ROBERTSON, edited with a memoir by Professor BAIN, is announced by Messrs. Putnam.

Messrs. Ginn & Co. announce the publication of an experimental study by Dr. HERBERT NICHOLS entitled *Our Notions of Number and Space*.

We are asked to print the following note :

"The editors of *Mind* request that all MSS. from America intended for publication in that Journal be sent to Professor TITCHENER, Cornell University, Ithaca, N. Y., instead of to Mr. STOUT as has hitherto been the case."